THE PHILOSOPHICAL MAGAZINE AND JOURNAL:

COMPREHENDING

THE VARIOUS BRANCHES OF SCIENCE,

THE LIBERAL AND FINE ARTS,

GEOLOGY,

AGRICULTURE,

MANUFACTURES AND COMMERCE.

BY ALEXANDER TILLOCH,

M.R.I.A. M.R.A.S. MUNICH, F.S.A. EDIN. AND PERTH; &c.

"Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." JUST. LIPS. *Monit. Polit.* lib. i. cap. 1.

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THE PHILOSOPHICAL MAGAZINE AND JOURNAL.

I. *Some new Researches on Flame.* By Sir HUMPHRY DAVY, LL.D. F.R.S. V.P.R.I.*

I HAVE described in three papers which the Royal Society have honoured with a place in their Transactions, a number of experiments on combustion, which show that the explosion of gaseous mixtures can be prevented or arrested by various cooling influences, and which led me to discover a tissue permeable to light and air, but impermeable to flame, on which I founded the invention of the wire-gauze safe-lamp now generally used in all collieries in which inflammable air prevails, for the preservation of the lives and persons of the miners. In a short notice published in the third number of the Journal of Science and the Arts, edited at the Royal Institution, I have given an account of some new results on flame, which show that the intensity of the light of flames depends principally upon the production and ignition of solid matter in combustion, and that the heat and light in this process are in a great measure independent phænomena. Since this notice has been printed, I have made a number of researches on flame: and as they appear to me to throw some new lights on this important subject, and to lead to some practical views connected with the useful arts, I shall without any further apology present them to the Royal Society.

That greater distinctness may exist in the details, I shall treat of my subjects under four heads. In the first I shall discuss the effects of rarefaction, by partly removing the pressure of the atmosphere upon flame and explosion. In the second, I shall consider the effects of heat in combustion. In the third, I shall examine the effect of the mixture of gaseous substances not concerned in combustion upon flame and explosion. In the fourth, I shall offer some general views upon flame, and point out certain practical and theoretical applications of the results.

* From the Transactions of the Royal Society for 1817, part i.

I. *On the Effect of Rarefaction by partly removing the Pressure of the Atmosp re upon Flame and Explosion.*

The earlier experimenters upon the Boylean vacuum observed that flame ceased in highly rarefied air: but the degree of rarefaction necessary for this effect has been differently stated. Amongst late experimenters, M. de Grotthus has examined this subject. He has asserted that a mixture of oxygen and hydrogen ceases to be explosive by the electrical spark when rarefied sixteen times, and that a mixture of chlorine and hydrogen cannot be exploded when rarefied only six times, and he generalizes by supposing that rarefaction, whether produced by removing pressure or by heat, has the same effect.

I shall not begin by discussing the experiments of this ingenious author. My own results and conclusions are very different from his; and the cause of this difference will I think be obvious in the course of these inquiries. I shall proceed in stating the observations which guided my researches.

When hydrogen gas slowly produced from a proper mixture was inflamed at a fine orifice of a glass tube, as in the experiment called the philosophical candle, so as to make a jet of flame of about 1-6th of an inch in height, and introduced under the receiver of an air-pump containing from 200 to 300 cubical inches of air, the flame enlarged as the receiver became exhausted; and, when the gauge indicated a pressure between four and five times less than that of the atmosphere, was at its maximum of size: it then gradually diminished below, but burned above, till the pressure was between seven and eight times less, when it became extinguished.

To ascertain whether the effect depended upon the deficiency of oxygen, I used a larger jet with the same apparatus, when the flame to my surprise burned longer, and when the atmosphere was rarefied ten times, and this in repeated trials. When the larger jet was used, the point of the glass tube became white hot, and continued red hot till the flame was extinguished. It immediately occurred to me, that the heat communicated to the gas by this tube, was the cause that the combustion continued longer in the last trials when the larger flame was used; and the following experiments confirmed the conclusion. A piece of wire of platinum was coiled round the top of the tube, so as to reach into and above the flame. The jet of gas of 1-6th of an inch in height was lighted and the exhaustion made; the wire of platinum soon became white hot in the centre of the flame, and a small point of wire near the top fused: it continued white hot till the pressure was six times less, when it was ten times it continued red hot at the upper part, and, as long as it was dull red,

red, the gas though extinguished below, continued to burn in contact with the hot wire, and the combustion did not cease until the pressure was reduced thirteen times.

It appears from this result, that the flame of hydrogen is extinguished in rarefied atmospheres, only when the heat it produces is insufficient to keep up the combustion, which appears to be when it is incapable of communicating visible ignition to metal; and as this is the temperature required for the inflammation of hydrogen at common pressures, it appears that its *combustibility* is neither diminished nor increased by rarefaction from the removal of pressure.

According to this view with respect to hydrogen, it should follow that amongst other combustible bodies, those which require least heat for their combustion, ought to burn in more rarefied air than those that require more heat, and those that produce much heat in their combustion ought to burn, other circumstances being the same, in more rarefied air than those that produce little heat: and every experiment I have made confirms these conclusions. Thus olefiant gas which approaches nearly to hydrogen in the heat produced by its combustion, and which does not require a much higher temperature for its inflammation, when its flame was made by a jet of gas from a bladder connected with a small tube furnished with a wire of platinum, under the same circumstances as hydrogen, ceased to burn when the pressure was diminished between ten and eleven times: and the flames of alcohol and of the wax taper which require a greater consumption of heat for the volatilization and decomposition of their combustible matter, were extinguished when the pressure was five or six times less without the wire of platinum, and seven or eight times less when the wire was kept in the flame. Light carburetted hydrogen, which produces, as will be seen hereafter, less heat in combustion than any of the common combustible gases, except carbonic oxide, and which requires a higher temperature for its inflammation than any other, had its flame extinguished, even though the tube was furnished with the wire when the pressure was below 1-4th.

The flame of carbonic oxide which, though it produces little heat in combustion, is as inflammable as hydrogen, burned when the wire was used, the pressure being 1-6th.

The flame of sulphuretted hydrogen, the heat of which is in some measure carried off by the sulphur produced by its decomposition during its combustion in rare air, when burned in the same apparatus as the olefiant and other gases, was extinguished when the pressure was 1-7th.

Sulphur, which requires a lower temperature for its combustion than any common inflammable substance, except phosphorus,

burned with a very feeble blue flame in air rarefied fifteen times, and at this pressure the flame heated a wire of platinum to dull redness, nor was it extinguished till the pressure was reduced to 1-20th*.

Phosphorus, as has been shown by M. Van Marum, burns in an atmosphere rarefied sixty times: and I found that phosphuretted hydrogen produced a flash of light when admitted into the best vacuum that could be made, by an excellent pump of Nairn's construction.

The mixture of chlorine and hydrogen inflames at a much lower temperature than that of hydrogen and oxygen, and produces a considerable degree of heat in combustion; it was therefore probable that it would bear a greater degree of rarefaction, without having its power of exploding destroyed; and this I found in many trials is actually the case, contrary to the assertion of M. de Grotthus. Oxygen and hydrogen in the proportion to form water, will not explode by the electrical spark when rarefied eighteen times; but hydrogen and chlorine in the proportion to form muriatic acid gas, gave a distinct flash of light under the same circumstances, and they combined with visible inflammation when the spark was passed through them, the exhaustion being to 1-24th.

The experiment on the flame of hydrogen with the wire of platinum, and which holds good with the flames of the other gases, shows, that by preserving heat in rarefied air, or giving heat to a mixture, inflammation may be continued when, under common circumstances, it would be extinguished. This I found was the case in other instances, when the heat was differently communicated: thus, when camphor was burned in a glass tube, so as to make the upper part of the tube red hot, the inflammation continued when the rarefaction was nine times, whereas it would only continue in air rarefied six times, when it was burned in a thick metallic tube which could not be considerably heated by it.

By bringing a little naphtha in contact with a red hot iron, it produced a faint lambent flame, when there remained in the receiver only 1-30th of the original quantity of air, though without foreign heat its flame was extinguished when the quantity was 1-6th.

* The temperature of the atmosphere diminishes in a certain ratio with its height, which must be attended to in the conclusions respecting combustion in the upper regions of the atmosphere, and the elevation must be somewhat lower than in arithmetical progression, the pressure decreasing in geometrical progression.

There is, however, every reason to believe, that the taper would be extinguished at a height of between nine and ten miles, hydrogen between twelve and thirteen, and sulphur between fifteen and sixteen.

I rarefied

I rarefied a mixture of oxygen and hydrogen by the air-pump to about eighteen times, when it could not be inflamed by the electric spark. I then heated strongly the upper part of the tube till the glass began to soften, and passed the spark, when a feeble flash was observed not reaching far into the tube, the heated gases only appearing to enter into inflammation. This last experiment requires considerable care. If the exhaustion is much greater, or if the heat is raised very slowly*, it does not succeed; and if the heat is raised so high as to make the glass luminous, the flash of light, which is extremely feeble, is not visible; it is difficult to procure the proper degree of exhaustion, and to give the exact degree of heat; I have, however, succeeded three times in obtaining the results, and in one instance it was witnessed by Mr. Brande.

To elucidate the inquiry still further, I made a series of experiments on the heat produced by some of the inflammable gases in combustion. In comparing the heat communicated to wires of platinum by flames of the same size, it was evident, that hydrogen and olefiant gas in oxygen, and hydrogen in chlorine, produced a much greater intensity of heat in combustion, than the other gaseous substances I have named burned in oxygen; but no regular scale could be formed from observations of this kind. I endeavoured to gain some approximations on the subject by burning equal quantities of different gases under the same circumstances, and applying the heat to an apparatus by which it could be measured. For this purpose a mercurial gas-holder was furnished with a system of stop-cocks, terminating in a strong tube of platinum having a minute aperture. Above this was fixed a copper cup filled with olive oil, in which a thermometer was placed. The oil was heated to 212° to prevent any differences in the communication of heat by the condensation of aqueous vapour; the pressure was the same for the different gases, and they were consumed as nearly as possible in the same time, and the flame applied to the same point of the copper cup, the bottom of which was wiped after each experiment.

The results were as follows:

The flame from	olefiant gas raised the thermometer to	270°
———	hydrogen	238
———	sulphuretted hydrogen	232
———	coal gas	236
———	gaseous oxide of carbon	218

The quantities of oxygen consumed (that absorbed by the hydrogen being taken as one) would be, supposing the combustion perfect, for the olefiant gas six, for the sulphuretted hydrogen

* The reason will be obvious from what is stated in page 9.

three, for the carbonic oxide one. The coal gas contained only a very small proportion of olefiant gas; supposing it to be pure carburetted hydrogen, it would have consumed four of oxygen. Taking the elevations of temperature, and the quantities of oxygen consumed as the data, the ratios of the heat produced by the combustion of the different gases, would be for hydrogen twenty-six, for olefiant gas 9·66, for sulphuretted hydrogen 6·66, for carburetted hydrogen six, for carbonic oxide six*.

It will be useless to reason upon this ratio as exact, for charcoal was deposited both from the olefiant gas and coal gas during the experiment, and much sulphur was deposited from the sulphuretted hydrogen; and there is great reason to believe, that the capacities of fluids for heat increase with their temperature. It confirms, however, the general conclusions, and proves that hydrogen stands at the head of the scale, and gaseous oxide of carbon at the bottom. It might at first view be imagined that, according to this scale, the flame of carbonic oxide ought to be extinguished by rarefaction, at the same degree as that of carburetted hydrogen; but it must be remembered, as I have mentioned in another place, that carbonic oxide is a much more combustible gas. Carbonic oxide inflames in the atmosphere when brought into contact with an iron wire heated to dull redness, whereas carburetted hydrogen is not inflammable by a similar wire, unless it is heated to whiteness so as to burn with sparks.

II. *On the Effects of Rarefaction by Heat on Combustion and Explosion.*

The results detailed in the preceding section are indirectly opposed to the opinion of M. de Grotthus, that rarefaction by heat destroys the combustibility of gaseous mixtures. Before I made any direct experiments on this subject, I endeavoured to ascertain the degree of expansion which can be communicated to elastic fluids by the strongest heat that can be applied to glass vessels. For this purpose I introduced into a graduated curved glass tube some fusible metal. I heated the fusible metal and the portion of the tube containing the air included by it, under boiling water for some time. I then placed the apparatus in a charcoal fire, and very gradually raised the temperature till the fusible metal appeared luminous when viewed in the shade. At this time the air had expanded so as to occupy 2·25 parts in the tube, it being one at the temperature of boiling water. Another experiment was made in a thicker glass tube, and the heat was

* These results may be compared with Mr. Dalton's new System of Chemical Philosophy; they agree in showing that hydrogen produces more heat in combustion than any of its compounds.

raised

raised until the tube began to run together; but though this heat appeared cherry red, the expansion was not to more than 2·5, and a part of this might perhaps have been apparent only, owing to the collapsing of the glass tube before it actually melted. It may be supposed that the oxidation of the fusible metal may have had some effect in making the expansion appear less; but in the first experiment the air was gradually brought back to its original temperature of boiling water, when the absorption was scarcely sensible. If M. Gay Lussac's conclusions be taken as the ground-work of calculation, and it be supposed that air expands equally for equal increments of temperature, it would appear that the temperature of air capable of rendering glass luminous must be 1035° Fahrenheit*.

M. de Grotthus describes an experiment in which atmospheric air and hydrogen, expanded to four times their bulk over mercury by heat, would not inflame by the electric spark. It is evident, that in this experiment a large quantity of steam or of mercurial vapour must have been present, which, like other inexplosive elastic fluids, prevents combustion when mixed in certain quantities with explosive mixtures; but though he seems aware that his gases were not dry, yet he draws his general conclusion, that expansion by heat destroys the explosive powers of gases, principally from this inconclusive experiment.

I introduced into a small graduated tube over well boiled mercury, a mixture of two parts of hydrogen and one of oxygen, and heated the tube by a large spirit-lamp till the volume of the gas was increased from 1 to 2·5. I then, by means of a blowpipe and another spirit-lamp, made the upper part of the tube red hot, when an explosion instantly took place.

I introduced into a bladder a mixture of oxygen and hydrogen, and connected this bladder with a thick glass tube of about 1-6th of an inch in diameter and three feet long, curved so that it could be gradually heated in a charcoal furnace; two spirit-lamps were placed under the tube where it entered the charcoal fire, and the mixture was very slowly pressed through: an explosion took place before the tube was red hot.

This experiment shows that expansion by heat, instead of diminishing the combustibility of gases, on the contrary, enables them to explode apparently at a lower temperature, which seems perfectly reasonable, as a part of the heat communicated by any ignited body must be lost in gradually raising the temperature.

* The mode of ascertaining temperatures as high as the point of fusion of glass by the expansion of air, seems more unexceptionable than any other. It gives for the point of visible ignition nearly the same degree as that deduced by Newton from the times of the cooling of ignited metal in the atmosphere.

I made

I made several other experiments which establish the same conclusions. A mixture of common air and hydrogen was introduced into a small copper tube, having a stopper not quite tight; the copper tube was placed in a charcoal fire: before it became visibly red an explosion took place, and the stopper was driven out.

I made various experiments on explosions by passing mixtures of hydrogen and oxygen through heated tubes: in the beginning of one of these trials, in which the heat was much below redness, steam appeared to be formed without any combustion. This led me to expose mixtures of oxygen and hydrogen in tubes, in which they were confined by fluid fusible metal to heat; and I found that by carefully applying a heat between the boiling point of mercury, which is not sufficient for the effect, and a heat approaching to the greatest heat that can be given without making glass luminous in darkness, the combination was effected without any violence, and without any light: and commencing with 212°, the volume of steam formed at the point of combination appeared exactly equal to that of the original gases. So that the first effect in experiments of this kind is an expansion, afterwards a contraction, and then the restoration of the primitive volume.

If when this change is going on, the heat be quickly raised to redness, an explosion takes place; but with small quantities of gas the change is completed in less than a minute.

It is probable, that the slow combination without combustion, already long ago observed with respect to hydrogen and chlorine, oxygen and metals, will happen at certain temperatures with most substances that unite by heat. On trying charcoal, I found that at a temperature which appeared to be a little above the boiling point of quicksilver, it converted oxygen pretty rapidly into carbonic acid, without any luminous appearance, and at a dull red heat, the elements of olefiant gas combined in a similar manner with oxygen, slowly and without explosion.

The effect of the slow combination of oxygen and hydrogen is not connected with their rarefaction by heat, for I found that it took place when the gases were confined in a tube by fusible metal rendered solid at its upper surface; and certainly as rapidly, and without any appearance of light.

M. de Grotthus has stated, that, if a glowing coal be brought into contact with a mixture of oxygen and hydrogen, it only rarefies them, but does not explode them; but this depends upon the degree of heat communicated by the coal: if it is red in day-light and free from ashes, it uniformly explodes the mixture; if its redness is barely visible in shade, it will not explode them, but cause their slow combination: and the general phænomenon is wholly unconnected with rarefaction, as is shown by the

the following circumstance. When the heat is greatest, and before the invisible combination is completed, if an iron wire heated to whiteness be placed upon the coal within the vessel, the mixture instantly explodes.

Light carburetted hydrogen, or pure fire-damp, as has been shown, requires a very strong heat for its inflammation; it therefore offered a good substance for an experiment on the effect of high degrees of rarefaction by heat on combustion. I mixed together one part of this gas and eight parts of air, and introduced them into a bladder furnished with a capillary tube. I heated this tube till it began to melt, and then slowly passed the mixture through it into the flame of a spirit-lamp, when it took fire and burned with its own peculiar explosive light beyond the flame of the lamp, and when withdrawn, though the aperture was quite white hot, it continued to burn vividly.

That the compression in one part of an explosive mixture produced by the sudden expansion of another part by heat, or the electric spark, is not the cause of combination, as has been supposed by Dr. Higgins, M. Berthollet, and others, appears to be evident from what has been stated, and it is rendered still more so by the following facts. A mixture of hydro-phosphoric gas (bi-phosphuretted hydrogen gas) and oxygen, which explode at a heat a little above that of boiling water, was confined by mercury, and very gradually heated on a sand-bath: when the temperature of the mercury was 242°, the mixture exploded.

A similar mixture was placed in a receiver communicating with a condensing syringe, and condensed over mercury till it occupied only 1-5th of its original volume. No explosion took place, and no chemical change had occurred; for when its volume was restored, it was instantly exploded by the spirit-lamp.

It would appear, then, that *the heat* given out by the compression of gases is the real cause of the combustion which it produces, and that at certain elevations of temperature, whether in rarefied or compressed atmospheres, explosion or combustion occurs, *i. e.* bodies combine with the production of heat and light.

III. *On the Effects of the Mixture of different Gases in Explosion and Combustion.*

In my first paper on the fire-damp of coal mines, I have mentioned that carbonic acid gas has a greater power of destroying the explosive power of mixtures of fire-damp and air than azote, and I have ventured to suppose the cause to be its greater density and capacity for heat, in consequence of which it might exert a greater cooling agency, and prevent the temperature of the mixture from being raised to that degree necessary for combustion.

bustion. I have lately made a series of experiments with the view of determining how far this idea is correct, and for the purpose of ascertaining the general phænomena of the effects of the mixture of gaseous substances upon explosion and combustion.

I took given volumes of a mixture of two parts of hydrogen and one part of oxygen by measure, and diluting them with various quantities of different elastic fluids, I ascertained at what degree of dilution the power of inflammation by a strong spark from a Leyden phial was destroyed. I found that for one of the mixture inflammation was prevented by

Of Hydrogen, about	8
Oxygen	9
Nitrous oxide	11
Carburetted hydrogen	1
Sulphuretted hydrogen	2
Olefiant gas	$\frac{1}{2}$
Muriatic acid gas	2
Silicated fluoric acid gas	$\frac{5}{6}$

Inflammation took place when the mixtures contained of

Hydrogen	6
Oxygen	7
Nitrous oxide	10
Carburetted hydrogen	$\frac{3}{4}$
Olefiant gas	$\frac{1}{3}$
Sulphuretted hydrogen	$1\frac{1}{2}$
Muriatic acid gas	$1\frac{1}{2}$
Fluoric acid gas	$\frac{3}{4}$

I hope to be able to repeat these experiments with more precision at no distant time; the results are not sufficiently exact to lay the foundation for any calculations on the relative cooling powers of equal volumes of the gases; but they show sufficiently, if the conclusions of MM. de la Roche and Berard be correct, that other causes, besides density and capacity for heat, interfere with the phænomena. Thus nitrous oxide, which is nearly 1-3d denser than oxygen, and which, according to De la Roche and Berard, has a greater capacity for heat in the ratio of 1·3503 to ·9765 in volume, has lower powers of preventing explosion; and hydrogen, which is fifteen times lighter than oxygen, and which in equal volumes has a smaller capacity for heat, certainly has a higher power of preventing explosion; and olefiant gas exceeds all other gaseous substances in a much higher ratio than could have been expected from its density and capacity. The olefiant gas I used was recently made, and might have contained some vapour of ether, and the nitrous oxide was mixed with some azote, but these slight causes could not have interfered with the results to any considerable extent.

Mr.

Mr. Leslie, in his elaborate and ingenious researches on heat, has observed the high powers of hydrogen of abstracting heat from solid bodies, as compared with that of common air and oxygen. I made a few experiments on the comparison of the powers of hydrogen, in this respect, with those of carburetted hydrogen, azote, oxygen, olefiant gas, nitrous oxide, chlorine, and carbonic acid gas. The same thermometer raised to the same temperature, 160°, was exposed to equal volumes (21 cubic inches) of olefiant gas, coal gas, carbonic acid gas, chlorine, nitrous oxide gas, hydrogen, oxygen, azote, and air, at equal temperatures, 52° Fahrenheit.

The times required for cooling to 106° were for

Air	2′ 0″	Oxygen	1′ 47″
Hydrogen	·45	Nitrous oxide* ..	2·30·2·55
Olefiant gas	1·15	Carbonic acid gas*	2·45
Coal gas	·55	Chlorine	3·6
Azote	1·30		

It appears from these experiments, that the power of elastic fluids to abstract or conduct away heat from solid surfaces, is in some inverse ratio to their density, and that there is something in the constitution of the light gases, which enables them to carry off heat from solid surfaces in a different manner from that in which they would abstract it in gaseous mixtures, depending probably upon the mobility of their parts†. The heating of gaseous media by the contact of fluid or solid bodies, as has been shown by Count Rumford, depends principally upon the change of place of their particles; and it is evident from the results stated in the beginning of this section, that these particles have different powers of abstracting heat analogous to the different powers of solids and fluids. Where an elastic fluid exerts a cooling influence on a solid surface, the effect must depend principally upon the rapidity with which its particles change their places: but where the cooling particles are mixed throughout a mass with other gaseous particles, their effect must principally depend upon the power they possess of rapidly abstracting heat from the contiguous particles; and this will depend probably upon two causes, the simple abstracting power by which they become quickly heated, and their capacity for heat, which is great in proportion as their temperatures are less raised by this abstraction.

* These two last results were observed by Mr. Faraday of the Royal Institution, (from whom I receive much useful assistance in most of my experiments,) when I was absent from the Laboratory.

† Those particles which are lightest must be conceived most capable of changing place, and would therefore cool solid surfaces most rapidly: in the cooling of gaseous mixtures, the mobility of the particles can be of little consequence.

Whatever

Whatever be the cause of the different cooling powers of the different elastic fluids in preventing inflammation, very simple experiments show that they operate uniformly with respect to the different species of combustion, and that those explosive mixtures, or inflammable bodies, which require least heat for their combustion, require larger quantities of the different gases to prevent the effect, and *vice versâ;* thus one of chlorine and one of hydrogen still inflame when mixed with eighteen times their bulk of oxygen, whereas a mixture of carburetted hydrogen and oxygen in the proper proportions for combinations, one and two, have their inflammation prevented by less than three times their volume of oxygen.

A wax taper was instantly extinguished in air mixed with 1-10th of silicated fluoric acid gas, and in air mixed with 1-6th of muriatic acid gas; but the flame of hydrogen burned readily in those mixtures, and in mixtures in which the flame of hydrogen was extinguished, the flame of sulphur burned.

There is a very simple experiment which demonstrates in an elegant manner this general principle. Into a long bottle with a narrow neck introduce a lighted taper, and let it burn till it is extinguished; carefully stop the bottle, and introduce another lighted taper, it will be extinguished before it reaches the bottom of the neck: then introduce a small tube containing zinc and diluted sulphuric acid, and at the aperture of which the hydrogen is inflamed; the hydrogen will be found to burn in whatever part of the bottle the tube is placed: after the hydrogen is extinguished, introduce lighted sulphur; this will burn for some time, and after its extinction, phosphorus will be as luminous as in the air, and, if heated in the bottle, will produce a pale yellow flame of considerable density.

In cases when the heat required for chemical union is very small, as in the instance of hydrogen and chlorine, a mixture which prevents inflammation will not prevent combination, *i. e.* the gases will combine without any flash. This I witnessed in mixing two volumes of carburetted hydrogen with one of chlorine and hydrogen; muriatic acid was formed throughout the mixture, and heat produced, as was evident from the expansion when the spark passed, and the rapid contraction afterwards, but the heat was so quickly carried off by the quantity of carburetted hydrogen that no flash was visible.

In the case of phosphorus, which is combustible at the lowest temperature of the atmosphere, no known admixture of elastic fluid prevents the luminous appearance; but this seems to depend upon the light being limited to the solid particles of phosphoric acid formed; whereas to produce flame, a certain mass of elastic fluid must be luminous; and there is every reason to believe,

believe, that when phosphuretted hydrogen explodes in very rare air, it is only the phosphorus which is consumed. Any other substance that produces solid matter in combustion would probably be luminous in air as rare, or in mixtures as diluted, as phosphorus, provided the heat was elevated sufficiently for its combustion. I have found that this is actually the case with respect to zinc. I threw some zinc filings into an ignited iron crucible fixed on the stand of an air-pump under a receiver, and exhausted until only 1-60th of the original quantity of air remained. When I judged that the red hot crucible must be full of the vapour of zinc, I admitted about 1-60th more of air, when a bright flash of light took place in and above the crucible, similar to that which is produced by admitting air to the vapour of phosphorus *in vacuo.*

The cooling power of mixtures of elastic fluids in preventing combustion must increase with their condensation, and diminish with their rarefaction; at the same time, the quantity of matter entering into combustion in given spaces, is relatively increased and diminished. The experiments on flame in rarefied atmospherical air, show that the quantity of heat produced in combustion is very slowly diminished by rarefaction, the diminution of the cooling power of the azote being apparently in a higher ratio than the diminution of the heating powers of the burning bodies. I endeavoured to ascertain what would be the effect of condensation on flame in atmospheric air, and whether the cooling power of the azote would increase in a lower ratio, as might be expected, than the heat produced by the increase of the quantity of matter entering into combustion; but I found considerable difficulties in making the experiments with precision. I ascertained, however, that both the light and heat of the flames of the taper, of sulphur and hydrogen, were increased by acting on them by air condensed four times; but not more than they would have been by an addition of 1-5th of oxygen.

I condensed air nearly five times, and ignited iron wire to whiteness in it by the voltaic apparatus; but the combustion took place with very little more brightness than in the common atmosphere, and would not continue as in oxygen, nor did charcoal burn much more brightly in this compressed air than in common air. I intend to repeat these experiments, if possible, with higher condensing powers: they show sufficiently that (for certain limits at least) as rarefaction does not diminish considerably the heat of flame in atmospherical air, so neither does condensation considerably increase it; a circumstance of great importance in the constitution of our atmosphere, which at all the heights or depths at which man can exist still preserves the same relations to combustion.

It

It may be concluded from the general law, that at high temperatures, gases not concerned in combustion will have less powers of preventing that operation, and likewise, that steam and vapours, which require a considerable heat for their formation, will have less effect in preventing combustion, particularly of those bodies requiring low temperatures, than gases at the common heat of the atmosphere.

I have made some experiments on the effects of steam, and their results were conformable to these views. I found that a very large quantity of steam was necessary to prevent sulphur from burning. Oxygen and hydrogen exploded by the electric spark when mixed with five times their volume of steam; and even a mixture of air and carburetted hydrogen gas, the least explosive of all mixtures, required a third of steam to prevent its explosion, whereas 1-5th of azote produced the effect. These trials were made over mercury; heat was applied to water above the mercury, and 37·5 for 100 parts was regarded as the correction for the expansion of the gases.

It is probable that with certain heated mixtures of gases, where the non-supporting or non-inflammable elastic fluids are in great quantities, combination with oxygen will take place, as in the instance mentioned, page 14, of hydrogen and chlorine, without any light, for the temperature produced will not be sufficient to render elastic media luminous; and there are no combustions, except those of the compounds of phosphorus and the metals, in which solid matters are the result of combinations with oxygen. I have shown in the paper referred to in the introduction, that the light of common flames depends almost entirely upon the deposition, ignition and combustion of solid charcoal; but to produce this deposition from gaseous substances demands a high temperature. Phosphorus, which rises in vapour at common temperatures, and the vapour of which combines with oxygen at those temperatures, as I have mentioned before, is always luminous, for each particle of acid formed must, there is every reason to believe, be white hot; but so few of these particles exist in a given space that they scarcely raise the temperature of a solid body exposed to them, though, as in the rapid combustion of phosphorus, where immense numbers are existing in a small space, they produce a most intense heat.

In all cases the quantity of heat communicated by combustion, will be in proportion to the quantity of burning matter coming in contact with the body to be heated. Thus, the blow-pipe and currents of air operate. In the atmosphere, the effect is impeded by the mixture of azote, though still it is very great: with pure oxygen compression produces an immense effect, and with currents of oxygen and hydrogen, there is every reason to believe

that

that solid matters are made to attain the temperature of the flame. This temperature, however, evidently presents the limit to experiments of this kind; for bodies exposed to flame can never be hotter than flame itself; whereas in the Voltaic apparatus there seems to be no limit to the heat, except the volatilization of the conductors.

The temperatures of flames are probably very different. Where, in chemical changes, there is no change of volume, as in the instance of the mutual action of chlorine and hydrogen, prussic gas (cyanogen) and oxygen, approximations to their temperatures may be gained from the expansion in explosion.

I have made some experiments of this kind by detonating the gases by the electrical spark in a curved tube containing mercury or water; and I judged of the expansion from the quantity of fluid thrown out of the tube: the resistance opposed by mercury, and its great cooling powers, rendered the results very unsatisfactory in the cases in which it was used; but with water, cyanogen and oxygen being employed, they were more conclusive. Cyanogen and oxygen, in the proportion of one to two, detonated in a tube of about 2-5ths of an inch in diameter, displaced a quantity of water which demonstrated an expansion of fifteen times their original bulk. This would indicate a temperature of above 5000° of Fahrenheit, and the real temperature is probably much higher; for heat must be lost by communication to the tube and the water. The heat of the gaseous carbon in combustion in this gas, appears more intense than that of hydrogen; for I found a filament of platinum was fused by a flame of cyanogen in the air which was not fused by a similar flame of hydrogen.

IV. *Some general Observations, and practical Inferences.*

The knowledge of the cooling power of elastic media in preventing the explosion of the fire-damp, led me to those practical researches which terminated in the discovery of the wire-gauze safe-lamp; and the general investigation of the relation and extent of these powers serves to elucidate the operation of wire-gauze and other tissues or systems of apertures permeable to light and air, in intercepting flame, and confirms the views I originally gave of the phænomenon.

Flame is gaseous matter heated so highly as to be luminous, and that to a degree of temperature beyond the white heat of solid bodies, as is shown by the circumstance, that air not luminous will communicate this degree of heat*. When an attempt is made to pass flame through a very fine mesh of wire-

* This is proved by the simple experiment of holding a fine wire of platinum about the 1-20th of an inch from the exterior of the middle of the flame of a spirit-lamp, and concealing the flame by an opaque body. The wire will become white hot in a space where there is no visible light.

gauze at the common temperature, the gauze cools each portion of the elastic matter that passes through it, so as to reduce its temperature below that degree at which it is luminous, and the diminution of temperature must be proportional to the smallness of the mesh and the mass of the metal. The power of a metallic or other tissue to prevent explosion, will depend upon the heat required to produce the combustion as compared with that acquired by the tissue; and the flame of the most inflammable substances, and of those that produce most heat in combustion, will pass through a metallic tissue that will interrupt the flame of less inflammable substances, or those that produce little heat in combustion. Or the tissue being the same, and impermeable to all flames at common temperatures, the flames of the most combustible substances, and of those which produce most heat, will most readily pass through it when it is heated, and each will pass through it at a different degree of temperature. In short, all the circumstances which apply to the effect of cooling mixtures upon flame, will apply to cooling perforated surfaces. Thus, the flame of phosphuretted hydrogen at common temperatures, will pass through a tissue sufficiently large not to be immediately choked up by the phosphoric acid formed, and the phosphorus deposited *. A tissue of 100 apertures to the square inch, made of wire of 1-60th, will at common temperatures intercept the flame of a spirit-lamp but not that of hydrogen; and when strongly heated, it will no longer arrest the flame of the spirit-lamp. A tissue which will not interrupt the flame of hydrogen when red hot, will still intercept that of olefiant gas; and a heated tissue which would communicate explosion from a mixture of olefiant gas and air, will stop an explosion from a mixture of fire-damp, or carburetted hydrogen.

The ratio of the combustibility of the different gaseous matters is likewise to a certain extent as the masses of heated matter required to inflame them †. Thus an iron wire of 1-40th of an inch heated cherry red, will not inflame olefiant gas, but it will inflame hydrogen gas; and a wire of 1-8th, heated to the same degree, will inflame olefiant gas; but a wire of 1-500dth must be heated to whiteness to inflame hydrogen, though at a low red heat it will inflame bi-phosphuretted gas; but wire of 1-40th

* If a tissue containing above 700 apertures to the square inch be held over the flame of phosphorus or phosphuretted hydrogen, it does not transmit the flame till it is sufficiently heated to enable the phosphorus to pass through it in vapour. Phosphuretted hydrogen is decomposed in flame, and acts exactly like phosphorus.

† It appeared to me in these experiments, that the worst conducting and best radiating substances required to be heated higher for equal masses to produce the same effect upon the gases: thus, red hot charcoal had evidently less power of inflammation than red hot iron.

heated

heated even to whiteness will not inflame mixtures of fire-damp.

These circumstances will explain, why a mesh of wire so much finer is required to prevent the explosion from hydrogen and oxygen from passing, and why so coarse a texture and wire is sufficient to prevent the explosion of the fire-damp, fortunately the least combustible of the known inflammable gases.

The general doctrine of the operation of wire-gauze cannot be better elucidated than in its effects upon the flame of sulphur. When wire-gauze of 600 or 700 apertures to the square inch is held over the flame, fumes of condensed sulphur immediately come through it, and the flame is intercepted; the fumes continue for some instants, but as the heat increases they diminish; and at the moment they disappear, which is long before the gauze becomes red hot, the flame passes; the temperature at which sulphur burns being that at which it is gaseous.

Another very simple illustration of the truth of this view is offered in the effect of the cooling agency of metallic surfaces upon very small flames. Let the smallest possible flame be made by a single thread of cotton immersed in oil, and burning immediately upon the surface of the oil: it will be found to be about 1-30th of an inch in diameter. Let a fine iron wire of 1-180th be made into a circle of 1-10th of an inch in diameter, and brought over the flame. Though at such a distance, it will instantly extinguish the flame, if it be *cold:* but if it be held above the flame, so as to be slightly heated, the flame may be passed through it without being extinguished. That the effect depends entirely upon the power of the metal to abstract the heat of flame, is shown by bringing a glass capillary ring of *the same* diameter and size over the flame; this being a much worse conductor of heat, will not extinguish it even when *cold*. If its size however be made greater, and its circumference smaller, it will act like the metallic wire, and require to be heated to prevent it from extinguishing the flame*.

Suppose a flame divided by the wire-gauze into smaller flames, each flame must be extinguished in passing its aperture till that aperture has attained a temperature sufficient to produce the permanent combustion of the explosive mixture.

A flame of sulphur may be made much smaller than that of hydrogen, that of hydrogen smaller than that of a wick fed with

* Let a small globe of metal 1-20th of an inch in diameter made by fusing the end of a wire be brought near a flame of 1-30th in diameter, it will extinguish it when cold at the distance of its own diameter; let it be heated, and the distance will diminish at which it produces the extinction; and at a white heat it does not extinguish it by actual contact, though at a dull red heat it immediately produces the effect.

oil,

oil, and that of a wick fed with oil smaller than that of carburetted hydrogen; and a ring of cool wire which instantly extinguishes the flame of carburetted hydrogen, only slightly diminishes the size of a flame of sulphur of the same dimensions.

Where rapid currents of explosive mixtures are made to act upon wire-gauze, it is of course much more rapidly heated; and therefore the same mesh which arrests the flames of explosive mixtures at rest, will suffer them to pass when in rapid motion; but by *increasing* the cooling surface by diminishing the size, or increasing the depth of the aperture, all *flames*, however rapid their motion, may be arrested. Precisely the same law applies to explosions acting in close vessels: very minute apertures when they are only few in number will permit explosions to pass, which are arrested by much larger apertures when they fill a whole surface. A small aperture was drilled at the bottom of a wire-gauze lamp in the cylindrical ring which confines the wire-gauze; this, though less than 1-18th of an inch in diameter, passed the flame and fired the external atmosphere, in consequence of the whole force of the explosion of the thin stratum of the mixture included within the cylinder driving the flame through the aperture; though, had the whole ring been composed of such apertures separated by wires, it would have been perfectly safe.

Nothing can demonstrate more decidedly than these simple facts and observations, that the interruption of flame by solid tissues permeable to light and air, depends upon no recondite or mysterious cause, but to their cooling powers, simply considered as such.

When a light included in a cage of wire-gauze is introduced into an explosive atmosphere of fire-damp at rest, the maximum of heat is soon obtained; the radiating power of the wire, and the cooling effect of the atmosphere, more efficient from the mixture of inflammable air, prevent it from ever arriving at a temperature equal to that of dull redness. In rapid currents of explosive mixtures of fire-damp, which heat common gauze to a higher temperature, twilled gauze, in which the radiating surface is considerably greater, and the circulation of air less, preserves an equal temperature. Indeed the heat communicated to the wire by combustion of the fire-damp in wire-gauze lamps, is completely in the power of the manufacturer; for by diminishing the apertures and increasing the mass of metal, or the radiating surface, it may be diminished to any extent.

I have lately had lamps made of thick twilled gauze of wires of 1-40th, sixteen to the warp, and thirty to the weft, which being riveted to the screw, cannot be displaced; from its flexibility it cannot be broken, and from its strength cannot be crushed, except by a very strong blow.

Even

Even in the common lamps the flexibility of the material has been found of great importance; and I could quote one instance of a dreadful accident having been prevented, which must have happened had any other material than wire-gauze been employed in the construction of the lamp: and how little difficulty has occurred in the practical application of the invention, is shown by the circumstance, that it has been now for ten months in the hands of hundreds of common miners in the most dangerous mines in Britain, during which time not a single accident has occurred where it has been employed, whilst in other mines, much less dangerous, where it has not yet been adopted, some lives have been lost, and many persons burned.

The facts stated in Section II. explain why so much more heat is obtained from fuel when it is burnt quickly; and they show that in all cases the temperature of the acting bodies should be kept as high as possible, not only because the general increment of heat is greater, but likewise, because those combinations are prevented which at lower temperatures take place without any considerable production of heat:—thus, in the Argand lamp, the Liverpool lamp, and in the best fire-places, the increase of effect does not depend merely upon the rapid current of air, but likewise upon the heat preserved by the arrangements of the materials of the chimney, and communicated to the matters entering into inflammation.

These facts likewise explain the methods by which temperature may be increased, and the limit to certain methods. Currents of flame, as it was stated in the last section, can never raise the heat of bodies exposed to them, higher than a certain degree, their own temperature; but by compression, there can be no doubt, the heat of flames from pure supporters and combustible matter may be greatly increased, probably in the ratio of their compression. In the blow-pipe of oxygen and hydrogen, the maximum of temperature is close to the aperture from which the gases are disengaged, *i. e.* where their density is greatest. Probably a degree of temperature far beyond any that has been yet attained may be produced by throwing the flame from compressed oxygen and hydrogen into the Voltaic arc, and thus combining the two most powerful agents for increasing temperature.

The circumstances mentioned in this paper, combined with those noticed in the paper on flame printed in Mr. Brande's Journal of Science and the Arts, explain the nature of the light of flames and their form. When in flames pure gaseous matter is burnt, the light is extremely feeble: the density of a common flame is proportional to the quantity of solid charcoal first deposited and afterwards burnt. The form of the flame is conical, because the

greatest heat is in the centre of the explosive mixture. In looking steadfastly at flame, the part where the combustible matter is volatilized is seen, and it appears dark, contrasted with the part in which it begins to burn, that is where it is so mixed with air as to become explosive. The heat diminishes towards the top of the flame, because in this part the quantity of oxygen is least. When the wick increases to a considerable size from collecting charcoal, it cools the flame by radiation, and prevents a proper quantity of air from mixing with its central part; in consequence, the charcoal thrown off from the top of the flame is only red hot, and the greater part of it escapes unconsumed.

The intensity of the light of flames in the atmosphere is increased by condensation, and diminished by rarefaction, apparently in a higher ratio than their heat; more particles capable of emitting light exist in the denser atmospheres, and yet most of these particles, in becoming capable of emitting light, absorb heat; which could not be the case in the condensation of a pure supporting medium.

The facts stated in Section I. show that the luminous appearances of shooting stars and meteors cannot be owing to any inflammation of *elastic* fluids, but must depend upon the ignition of solid bodies. Dr. Halley calculated the height of a meteor at ninety miles, and the great American meteor which threw down showers of stones was estimated at seventeen miles high. The velocity of motion of these bodies must in all cases be immensely great, and the heat produced by the compression of the most rarefied air from the velocity of motion must be probably sufficient to ignite the mass; and all the phænomena may be explained, if *falling stars* be supposed to be small solid bodies moving round the earth in very eccentric orbits, which become ignited only when they pass with immense velocity through the upper regions of the atmosphere, and if the *meteoric bodies* which throw down stones with explosions be supposed to be similar bodies which contain either combustible or elastic matter.

Cobham-hall, Kent, Jan. 8, 1817.

Some new Experiments and Observations on the Combustion of Gaseous Mixtures, &c.

In a paper read before the Royal Society at their last two meetings, I have described the phænomena of the slow combustion of hydrogen and olefiant gas without flame. In the same paper I have shown, that the temperature of flame is infinitely higher than that necessary for the ignition of solid bodies. It appeared to me, therefore, probable, that in certain combinations of gaseous bodies, for instance, those above referred to, when the

the increase of temperature was not sufficient to render the gaseous matters themselves luminous; yet still it might be adequate to ignite solid matters exposed to them. I had devised several experiments on this subject. I had intended to expose fine wires to oxygen and olefiant gas, and to oxygen and hydrogen during their slow combination under different circumstances, when I was accidentally led to the knowledge of the *fact*, and, at the same time, to the discovery of a new and curious series of phænomena.

I was making experiments on the increase of the limits of the combustibility of gaseous mixtures of coal gas and air by increase of temperature. For this purpose, I introduced a small wire-gauze safe-lamp with some fine wire of platinum fixed above the flame, into a combustible mixture containing the maximum of coal gas; and when the inflammation had taken place in the wire-gauze cylinder, I threw in more coal gas, expecting that the heat acquired by the mixed gas in passing through the wire-gauze would prevent the excess from extinguishing the flame. The flame continued for two or three seconds after the coal gas was introduced; and when it was extinguished, that part of the wire of platinum which had been hottest remained ignited, and continued so for many minutes, and when it was removed into a dark room, it was evident that there was no flame in the cylinder.

It was immediately obvious that this was the result which I had hoped to attain by other methods, and that the oxygen and coal gas in contact with the hot wire combined without flame, and yet produced heat enough to preserve the wire ignited, and to keep up their own combustion. I proved the truth of this conclusion by making a similar mixture, heating a fine wire of platinum and introducing it into the mixture. It immediately became ignited nearly to whiteness, as if it had been itself in actual combustion, and continued glowing for a long while; and when it was extinguished, the inflammability of the mixture was found entirely destroyed.

A temperature much below ignition only was necessary for producing this curious phænomenon, and the wire was repeatedly taken out and cooled in the atmosphere till it ceased to be visibly red; and yet when admitted again, it instantly became red hot.

The same phænomena were produced with mixtures of olefiant gas and air, carbonic oxide, prussic gas and hydrogen, and in the last case with a rapid production of water; and the degree of heat I found could be regulated by the thickness of the wire. The wire, when of the same thickness, became more ignited in hydrogen than in mixtures of olefiant gas, and more in mixtures of olefiant gas than in those of gaseous oxide of carbon.

 When

When the wire was very fine, about the 1-80th of an inch in diameter, its heat increased in very combustible mixtures, so as to explode them. The same wire in less combustible mixtures only continued bright red, or dull red, according to the nature of the mixture.

In mixtures not explosive by flame within certain limits, these curious phænomena took place whether the air or the inflammable gas was in excess.

The same circumstance occurred with certain inflammable vapours. I have tried those of ether, alcohol, oil of turpentine and naphtha. There cannot be a better mode of illustrating the fact, than by an experiment on the vapour of ether or of alcohol, which any person may make in a minute. Let a drop of ether be thrown into a cold glass, or a drop of alcohol into a warm one. Let a few coils of wire of platinum of the 1-60th or 1-70th of an inch be heated at a hot poker or a candle, and let it be brought into the glass; it will in some part of the glass become glowing, almost white hot, and will continue so as long as a sufficient quantity of vapour and of air remain in the glass.

When the experiment on the slow combustion of ether is made in the dark, a pale phosphorescent light is perceived above the wire, which of course is most distinct when the wire ceases to be ignited. This appearance is connected with the formation of a peculiar acrid volatile substance possessed of acid properties.

The chemical changes in general produced by slow combustion appear worthy of investigation. A wire of platinum introduced under the usual circumstances into a mixture of prussic gas (cyanogen) and oxygen in excess became ignited to whiteness, and the yellow vapours of nitrous acid were observed in the mixture. And in a mixture of olefiant gas non-explosive from the excess of inflammable gas, much carbonic oxide was formed.

I have tried to produce these phænomena with various metals; but I have succeeded only with platinum and palladium; with copper, silver, iron, gold, and zinc, the effect is not produced. Platinum and palladium have low conducting powers, and small capacities for heat, compared with other metals; and these seem to be the principal causes of their producing, continuing, and rendering sensible these slow combustions.

I have tried some earthy substances which are bad conductors of heat; but their capacities and power of radiating heat appear to interfere. A thin film of carbonaceous matter entirely destroys the igniting power of platinum, and a slight coating of sulphuret deprives palladium of this property, which must principally depend upon their increasing the power of the metals to radiate heat.

Thin laminæ of the metals, if their form admits of a free circulation

culation of air, answer as well as fine wires; and a large surface of platinum may be made red hot in the vapour of ether, or in a combustible mixture of coal gas and air.

I need not dwell upon the connection of these facts respecting slow combustion, with the other facts I have described in the history of flame. Many theoretical views will arise from this connection, and hints for new researches, which I hope to be able to pursue in another communication. I shall now conclude by a practical application. By hanging some coils of fine wire of platinum, or a fine sheet of platinum or palladium, above the wick of his lamp, in the wire-gauze cylinder, the coal miner, there is every reason to believe, will be supplied with light in mixtures of fire-damp no longer explosive; and should his flame be extinguished by the quantity of fire-damp, the glow of the metal will continue to guide him; and by placing the lamp in different parts of the gallery, the relative brightness of the wire will show the state of the atmosphere in these parts. Nor can there be any danger with respect to respiration whenever the wire continues ignited, for even this phænomenon ceases when the foul air forms about 2-5ths of the volume of the atmosphere.

I introduced into a wire-gauze safe-lamp a small cage made of fine wire of platinum of the 1-70th of an inch in thickness, and fixed it by means of a thick wire of platinum about two inches above the wick which was lighted. I placed the whole apparatus in a large receiver, in which, by means of a gas-holder, the air could be contaminated to any extent with coal gas. As soon as there was a slight admixture of coal gas, the platinum became ignited; the ignition continued to increase till the flame of the wick was extinguished, and till the whole cylinder became filled with flame; it then diminished. When the quantity of coal gas was increased so as to extinguish the flame; at the moment of the extinction the cage of platinum became white hot, and presented a most brilliant light. By increasing the quantity of the coal gas still further, the ignition of the platinum became less vivid. When its light was barely sensible, small quantities of air were admitted, its heat speedily increased; and by regulating the admission of coal gas and air it again became white hot, and soon after lighted the flame in the cylinder, which as usual, by the addition of more atmospherical air, re-kindled the flame of the wick.

This experiment has been very often repeated, and always with the same results. When the wire for the support of the cage, whether of platinum, silver, or copper, was very thick, it retained sufficient heat to enable the fine platinum wire to rekindle in a proper mixture a half a minute after its light had been

been entirely destroyed by an atmosphere of pure coal gas; and by increasing its thickness the period might be made still longer.

The phænomenon of the ignition of the platinum takes place feebly in a mixture consisting of two of air and one of coal gas, and brilliantly in a mixture consisting of three of air and one of coal gas: the greater the quantity of heat produced the greater may be the quantity of the coal gas, so that a large tissue of wire will burn in a more inflammable mixture than single filaments, and a wire made white hot will burn in a more inflammable mixture than one made red hot. If a mixture of three parts of air and one of fire-damp be introduced into a bottle, and inflamed at its point of contact with the atmosphere, it will not explode, but will burn like a pure inflammable substance. If a fine wire of platinum coiled at its end be slowly passed through the flame, it will continue ignited in the body of the mixture, and the same gaseous matter will be found to be inflammable and to support combustion.

There is every reason to hope that the same phænomena will occur with the cage of platinum in the fire-damp, as those which have been described in its operation on mixtures of coal gas. In trying experiments in fire-damp, the greatest care must be taken that no filament or wire of platinum protrudes on the exterior of the lamp, for this would fire externally an explosive mixture. However small the mass of platinum which kindles an explosive mixture in the safe-lamp, the result is the same as when large masses are used; the force of the explosion is directed to, and the flame arrested by, the whole of the perforated tissue.

When a large cage of wire of platinum is introduced into a very small safe-lamp, even explosive mixtures of fire-damp are burnt without flame; and by placing any cage of platinum in the bottom of the lamp round the wick, the wire is prevented from being smoked. I have sent lamps furnished with this apparatus to be tried in the coal mines of Newcastle and Whitehaven: and I anxiously wait for the accounts of their effects in atmospheres in which no other permanent light can be produced by combustion.

London, Jan. 22, 1817.

Explanation of Figures, Plate I.

Fig. A is a small cage made of wire of platinum, of 1-70th or 1-80th of an inch in thickness, fastened to a wire for raising it above the wick, for giving light in inflammable media, containing too little air to be explosive.

Figures B and B are a similar cage for placing in the bottom of the lamp, to prevent it from being smoked by the wick.

II. *On*

II. *On Aërial Navigation.* *By Sir* George Cayley, *Bart.*

To Mr. Tilloch.

Sir, — Since my last paper on Aërial Navigation, several scattered observations have been made upon this subject in your Magazine; and although it has not met with all the encouragement it deserves, yet it has received as much notice as can reasonably be expected, when it is considered that it invites its supporters to a subscription, during an unparalleled period of public pecuniary privation. I am glad to find that a gentleman of distinguished literary and scientific reputation has stated to you his intention of subscribing fifty pounds towards any experiments on this subject, that may be conducted by men of science; alluding, I conceive, to the committee proposed in one of my papers. Mr. Evans has likewise signified his intention of subscribing, in conjunction with Mr. Lovell Edgeworth* and myself. It therefore becomes necessary to publish the present amount of the subscriptions, which I propose, subject to the permission of these two gentlemen, may be done in your Magazine for July; by which time I hope a few more names may be added, and a fund for experiments on the improvement of balloons be commenced, which will in time enable the capabilities of this interesting invention to be properly investigated and ascertained, under the inspection of a committee of scientific persons, acting with the advice of the best professional engineers in the country †. Surely, when it is considered that this leading discovery of suspending heavy bodies in the air by balloons is but recent in our age; and that the cumbrous and expensive nature of their structure has placed the proper scale of experiments far beyond the expense that individuals choose to appropriate to such purposes,—it cannot be deemed absurd, or even unworthy a sense of national pride, by a combined effort of intelligence and contribution, to rescue this noble invention from for ever remaining a gaudy bubble in the hands of exhibition-makers. All that I ask of men of information upon matters of this nature is, to combine, and to try such rational experiments, as would show by degrees

* Sir George will have learnt by this time that the gentleman whom he here names is now no more. He was the gentleman who had agreed to subscribe fifty pounds.—Edit.

† I stated last year to Mr. Tilloch the amount of my subscription, as the original promoter, under certain conditions: for the present I shall say 50 pounds; but I by no means wish gentlemen disposed to forward experiments on this subject to subscribe upon a high scale, as a greater amount may probably be obtained in subscriptions of from one to ten pounds.

how

how far it is practicable to guide balloons:—such a committee as I propose would never enter into any of those projects which, whether ultimately false or true, are at present too many steps in advance to be proper objects of their immediate attention; but, commencing with what has been ascertained upon this subject, would advance step by step from that point, as far as the present state of our knowledge of first moving powers will permit.

The title and terms of the subscription I therefore propose to be as follows:

WE, the undersigned parties, enter into the following subscription, for the purpose of ascertaining how far the principle of balloons supporting heavy burthens in the air may *be made* useful as a means of conveyance.

No person to be called upon for his subscription money till at least 1000*l.* be subscribed for.

When the subscription has reached this amount, an annual committee of seven of the subscribers to be elected;—every subscriber of one pound and of less than five pounds to have one vote on this and all other occasions. Subscribers of five pounds to have two votes; and subscribers of larger sums to have one additional vote for every additional five pounds they subscribe.

No experiments to be undertaken but by order of this committee, who may call in the advice of such civil engineers as they choose to consult.

An annual report of the application of the fund, and the result of the experiments made, to be printed for the use of the subscribers.

These regulations being the basis upon which the subscription is made, cannot be altered; but subsequent rules not militating against these, may be entered into at a general meeting of the subscribers, expressly convened for the purpose.

Having now stated my sentiments respecting the general bearing of this subject, I proceed to notice some remarks that have been made by others since my late papers. Mr. Evans has suggested as an improvement upon the triple tier of wing waftage by the steam-engine, that a rotary movement with oblique surfaces will be preferable, on account of the continual loss of power which he conceives to take place in putting these surfaces into motion from a state of rest. This reasoning against reciprocating movements is in general perfectly correct, but in this case the maxim does not hold good. The whole power communicated to these wafting surfaces is applied in the commencement to

to overcome the *vis inertiæ* of the materials of which they are composed, and the gradually increasing resistance of the air. Towards the termination of the waft, if the movement be properly contrived, the momentum accumulated in these surfaces will prolong the effective waft as much beyond the time when the effort of the first mover has ceased, as will exactly restore the power absorbed at the commencement of the action. Thus the whole power will have been expended on the resistance of the air, and consequently in propelling the balloon.

There are several difficulties of construction which occur in rotative wafts; the chief of which are, giving firm support and communicating motion to the axis at the necessary distance it is obliged to be placed from the boat; whereas in the wing waftage the hinge is on the solid frame of the boat. The wing construction likewise offers an advantage of great importance,—that of providing, if properly managed, a safe descent in case of accident to the balloon. The chief advantage of the rotary movement is its uniform action. I think either construction may be made effectual, but I prefer the wing plan as the easiest for our first experiments. Mr. Evans may see in my early papers upon this subject, that revolving flyers had not escaped my attention: indeed, the first experiment I made upon the mechanical principles of aërial navigation, was successfully executed, though on a very small scale and by very simple means, upon this very plan*.

Some very ingenious observations on the subject of aërial navigation are made by a correspondent in your Magazine for March 1817. In the third paragraph, respecting the means of vertical motion, the plan of condensing air into a second balloon is adverted to as worthy of particular attention. This plan of increasing the specific gravity by condensation, and lessening it again by the escape of the condensed air, was one of the earliest suggestions of the balloon-makers; but, though founded on a true principle, is quite inefficient in practice. The elastic pressure of air increasing as its density, no cloth is able to bear the force required: for instance, if a cloth be capable of resisting a lineal tension of five hundred pounds to the foot, let a balloon twenty feet in diameter be constructed of this cloth; it will readily be found upon calculation, that only from seventeen to twenty pounds of additional air can be pumped into it before it would arrive at the proposed tension. Thus a huge impediment to motion would be added to the machine, besides the additional bulk of the supporting balloon necessary to carry the weight of this incumbrance, without gaining any efficient power to compensate for these disadvantages.

* Nicholson's Journal for November 1809, p. 172.

In

In the second paragraph, respecting lateral motion, it is observed that the tacking plan, though worthy of much consideration, is incapable of counteracting any considerable wind, "as a little calculation will show." I must here remark, that if your correspondent will honour with his attention my statement respecting a Montgolfian balloon constructed on the tacking plan, in your Magazine for March 1816, and will recalculate the powers of that construction, he will find that the horizontal speed will be about twenty miles per hour in calm air; but he must not, as he proposes, consider the major axis as elevated in an angle of 45° with the horizon; but at an angle of 30°, which will be found to cause the path of the machine to be in the former angle; 15° or 16° being lost, in what is similar to lee-way in ships, according to the flatness of the top surface of the balloon. Although a velocity of twenty miles per hour will not overcome some winds, and would scarcely be at par with what Mr. Smeaton calls "very brisk" in his table; yet it would overcome what he terms "gently pleasant," at a speed of sixteen miles per hour; and what he terms "pleasant brisk," at about seven and a half. Very few days in the year have what is thus called very brisk wind, and it is even in this case 32 to 1 that it does not blow from that point of the compass which is the proposed direction of steerage. In most oblique cases the power of the machine will give a great command of diagonal steerage within the semicircle opposed to the wind; on either side it will be no impediment; and in the whole semicircle behind the wind it will add to the velocity required. Hence, as on most occasions a choice of time is left, winds will be of infinite use in aërial navigation, even should twenty miles per hour, in calm air, prove to be the limit to the velocity of these machines. The difference of the currents in the upper and lower strata of the atmosphere, it is well observed by your correspondent, will lend great assistance to the steerage of balloons, as will also the singular fact of their following the direction of rivers, which is probably an electric phænomenon, rivers acting like discharging rods by connecting the opposite electrical states of distant regions of the atmosphere, as is exemplified by the greater frequency of accidents from lightning on their banks than in ordinary situations.

In the third paragraph your correspondent states the failure of oars in moving balloons to have arisen from their being applied to the car, in lieu of "their line of pressure passing through the centre of pressure of the whole system," much of the power being thus applied towards communicating a rotary movement of the car round the balloon. I do not conceive this to be the cause

cause of failure, but the application of the power of one or two men, with very ill appropriated means, to perform what required the strength of twice as many horses. With respect to the oblique force noticed by your correspondent, I wish to refer him to the case of a barge drawn along the centre of a canal by a rope to a horse on the bank;—no power is lost by this mode of draft, but what arises from the actual path of the vessel not coinciding with the line of its major axis, which slight increase of resistance is foreign to the case of a spherical balloon, where simple gravitation, and not the pressure of a fluid on an oblique plane, is the restraining force. This is best explained by a figure.

Let A, fig. 1, Plate I. be a balloon. B its car, propelled beyond the centre of suspension by any given power of waftage; draw AC perpendicular, and CB parallel to the horizon; and let these lines be in the same ratio to each other as the weight of the car is to the propelling power; then the line AB will represent the whole action of the car upon the balloon. Draw AD and BD, respectively, parallel to the two former lines, and it becomes evident that the power of the compound force AB, will have the same effect as the two forces AD, equal to CB, the propelling power, and AC the weight of the car; which being just balanced by the floating power that may be represented by BD, leaves the balloon to be carried along in its horizontal path by the same force, as if dragged in the direct line of its centre AD. I have been the more particular in my observations on this point, because I wish to show that long balloons filled with hydrogen gas may be made use of at any distance above the car they support, which may be found to render them safe from the fire of the engine, and yet not be subject to any loss of power from the waftage being applied to the car in lieu of the balloon. Thirty or forty yards, if necessary, may intervene between the balloon and top of the chimney of the fire which works the engine. Wire-gauze, so celebrated of late for preventing the communication even of explosive mixtures of hydrogen with each other, may interpose its magic web to cut off any danger in this respect; and as the hydrogen gas balloon must (for the sake of firm resistance to the external air, so as to preserve the proper form of the prow) be inclosed in one of coarser materials, into which common air can be pumped to the required density between them, it becomes almost impossible that any accident from fire can take place. A flexible leather tube and cordage will thus form the only connection between the boat and the balloon. The stupendous bulk of such balloons as upon calculation appear capable of being made to convey considerable burthens with the requisite degree of speed, forms the chief

chief obstacle to their introduction. This causes the expense attending their structure and inflation, their tremendous power if assailed by winds, and the difficulty of disposing of them when not employed. The expense of structure would at present be about 300*l.* per ton; but if these vessels became of general utility, a much cheaper means of structure would probably soon be found out. The expense of inflating them with hydrogen gas is heavy by the present process; but as water consists of rather more than a sixth part of its weight of pure hydrogen *; and as every portion of hydrogen according to its purity gives from ten to twelve times its own weight of support in a balloon, it follows that every ton of water that is decomposed for this purpose will suspend very nearly two tons of burthen in the air. If this process, as I before suggested, be performed by exposing red hot iron to the action of steam, it appears, from the known proportion of oxygen in the black oxide thus formed †, that it will take about a ton and a half of iron to each ton of supporting power; and hence an oven of three and a half yards cubed will contain sufficient iron drops or borings, allowing one half of the space for the free passage of the steam amongst them, to inflate the balloon I have described of fifty tons power. As the oxide will be reduced by melting the iron again in the ordinary way, no metal would be lost; and the process would not be expensive if conducted where coal and iron ore are found together, as is frequently the case in this kingdom.

Charcoal will decompose water more rapidly and at a cheaper rate; and although the carburetted hydrogen thus obtained is generally much too heavy for inflating balloons; yet as the compound nature of this gas seems to vary according to the quantity and circumstances under which the steam comes in contact with the ignited charcoal; and as Lavoisier and Meusnier obtained it at the specific gravity of 0·279, air being ·1000, or rather more than three and a half times lighter than air, it is very probable that some ready mode may be found of obtaining pure hydrogen by the simple action of combustibles upon steam, which will render the floatage of balloons cheap enough for that ordinary use which, sooner or later, this principle was designed to be of to mankind. Had hydrogen been a scanty substance, to be found with difficulty, its remarkable levity, though attractive as a matter of curious chemical research, would only have been tantalizing, as exhibiting a means of suspending heavy bodies in

*		†	
85	Oxygen.	27	Oxygen.
15	Hydrogen.	73	Iron.
100	Water.	100	Black oxide.

the

the air; but who will deny that in chemistry, as in every other branch of natural knowledge, there exist palpable evidences of design and adaptation, either of man to these elements, or of the elements to the uses of man. I do not here allude to those self-evident and immediate adaptations, such as light to the eye, the structure of the lungs to the air we breathe, or of the stomach to the water we drink; but those more indirectly adapted to the pleasures, wants and conveniences of life: for instance, iron, which is certainly the most useful of the metals, is the most plentiful; its power of being made into steel for tools, capable, by so simple an art as that of being suddenly cooled, of acquiring any degree of hardness, so as even to cut steel itself;—the extraordinary power it has of becoming so far in a state of fusion as to admit of being perfectly united under the hammer in a welding heat, without losing the form it had been previously wrought into, are, in the opinion of every enlightened workman, evidences of design in its chemical structure as respecting the wants of mankind. No one can doubt that water, which seems to form the basis of all the vegetable and animal juices, was likewise designed as furnishing the means of navigation. Nature is no niggard of that which she designs for the uses of her creatures. The sun, in lighting up our enamelled acres, far outdoes the utmost brilliancy of our nocturnal ball-rooms; and to hire an acre of illumination equal to what this luminary bestows upon it gratis, would cost from thirty to forty thousand per annum. The very circumstance that every ton of water contains a power of giving two tons of floatage to heavy bodies within the atmosphere, is strong evidence that this may be intended as one of the uses of the chemical arrangement of this plentiful element.

The relative power of balloons to break away from their anchorage in a storm of wind, decreases under the circumstances of magnitude and oblong structure I have proposed, in the same ratio with the decrease of their resistance in passing through the air. The horizontal drag of the balloon of fifty tons when at anchor, and exposed to the various degrees of wind in Mr. Smeaton's table, will be as follows:

	Miles per Hour.	Tons.
High wind	$32\frac{1}{2}$	8
Very high wind	$42\frac{1}{2}$	$13\frac{1}{2}$
Storm or tempest	50	19
Great storm	60	27

Hence, even in the great storm, if the boat be anchored to the earth, the wind would only cause the connecting ropes to incline back to an angle of 33° with a perpendicular, and by no means overcome the floating power and beat the balloon to the earth so as to endanger it; provided the strength of the materials were

were such as to bear intense condensation sufficient to preserve the form of the prow under this load of pressure. This necessity of balloons to bear considerable internal and external pressure will oblige these machines to be made of strong materials, and to be braced by a wide net of cordage. It will likewise be necessary to make them in several compartments, like the stomachs of a leech, the power of the same cloth to resist condensation being inversely as the diameters of the containing bag. This additional weight will of course in the same degree diminish the supporting power: however, it may be practicable by means of tubes to each compartment, the mouths of which open externally to any required portion of the whole direct resistance of the wind, so to proportion the internal pressure, as only slightly to exceed the external in these respective compartments, and thus much of the strain may be avoided. The pressure of the atmosphere upon the skin of a moderate sized man amounts to about eight tons; but being balanced by an internal elasticity of equal amount, his lungs play without difficulty, and no strain is felt on any part of his skin. The necessity of having several compartments in large balloons, though an evil as to weight, is fully compensated for by the additional security it bestows:—by this structure, an accidental rupture of one portion would not cause a precipitate descent, as the floatage may be restored by a commensurate discharge of ballast, or of goods, in case of personal danger to the crew. The front or prow portion may be made of the strongest materials, and the hinder and middle portion of those duly proportioned to the stress they have to sustain; whereas, if all the air be in one vessel, every part must be alike capable of bearing the strongest strain. I would not have entered so minutely into these points, so much in advance of the present experimental state of the subject, were it not that the reluctance that is felt by some persons to aid experiments upon balloons, arises from a hasty conviction that the difficulties attending this subject are so great as to preclude all hopes of ultimately overcoming them: I wish to allow all the obstacles their fair weight, but to meet them by such expedients as their nature permits of, in doing which I fear I may have already trespassed too much upon your pages; and shall therefore conclude this paper with a very brief enumeration of the leading points that ought to induce experiments upon balloons to be made. They offer a direct swift and easy floatage from any one point to every other on the face of our globe.* Their relative resistance decreases inversely to their power of support; so that the large balloon of fifty tons formerly described, will meet with no more resistance than the bird from which its form is taken, weight for weight. Every ton of decomposed water gives two tons of floating

ing power. They would keep aloft, and be firm and steady in their position under anchorage, even in storms. The large balloon described, would pack up when out of use in a chamber within the boat eight yards by four, and thus render the apparatus compact on shore; and in the atmosphere there is unlimited space to accommodate any bulk with equal ease, especially when it is considered that every increase of it implies an increase of levity, and not of weight. Their structure being double, like a leathern foot-ball containing a bladder, the thin silken bag of hydrogen would not be exposed to any violence; and this gas being compressed on all sides alike by the condensed air surrounding it, would have no tendency to escape, during the action of the wind on the prow, as it would in the case of a common balloon, if at anchor or swiftly impelled through the air.

Danger from fire may be nearly excluded by the proper precautions. The same power that creates their progressive horizontal motion will effect their elevation and depression, by the application of an horizontal rudder or sail, and their steerage to either side by a vertical one. This will easily be understood from the sketch, fig. 2, Plate I. which represents a side view of the arrangement of the moving and steering sails of a balloon on the wing plan. Fig. 3 represents an end-view of a balloon with rotary flyers. Neither of these sketches shows any of the connecting parts belonging to their movements, which would have made the drawing confused.

I remain, sir,

Your obliged and obedient servant,

Brompton, May 12, 1817. GEO. CAYLEY.

III. *Remarks on Sir* RICHARD PHILLIPS'S *New Hypothesis. By* THOMAS TREDGOLD, *Esq.*

"——— He his fabric of the heavens
Hath left to their disputes, perhaps to move
His laughter at their quaint opinions wide."—*Milton.*

To Mr. Tilloch.

SIR, — A SLIGHT consideration must convince any person, that the phænomena of the universe cannot be the result of any continued chain of mechanical causes; and that, ultimately, we must arrive at some elements and powers or properties which can only be referred to the First Cause, "which certainly is not mechanical."

Reasoning on mechanical principles can be applied only to

 discover

discover the proportional effects of modified causes—all such reasoning being made on the presupposition of some active powers—which we know from experience, will produce the same effects under the same circumstances,—and when philosophical inquirers have traced all the phænomena of Nature to these original elements and powers, physical science may then be considered in its most perfect state. These elements and powers being the first principles of physical science, the combination and modification of them producing all the phænomena of Nature, it is desirable that they should be free from every thing that even has the appearance of being assumed without a sufficient foundation.

Attraction is one of those principles which have from time to time raised the scruples of philosophical inquirers, and particularly that kind of attraction which Newtonians call gravitation. The cause of attraction—if it has any other than the fiat of the Creator—appears to be placed beyond the powers of the human understanding; but its existence is proved by an abundant class of phænomena. That bodies attract each other when in contact few will be inclined to doubt;—but this being admitted, is any new difficulty created by supposing them to act at a distance? Suppose two bodies in contact are held together by attraction, why should an infinitely small distance totally destroy this force? Is it not more probable that the power decreases inversely as some function of the distance, than that it should abruptly cease at the instant of separation? Is it not proved by magnetical, electrical, chemical, and optical experiments, that attraction operates when bodies are not in contact? and, does not gravitation afford a satisfactory solution of the various phænomena of the solar system? which is not to be obtained by the introduction of any other principle whatever. This your correspondent Sir Richard Phillips is disposed to deny; and imagines that he has discovered the mechanical cause of the phænomena that appear to be the result of attraction. But his demonstrations, if such they can be called, are certainly of a very questionable nature.

Sir Richard takes it for granted, that the earth is moving in its orbit—but does not seem to be aware that attraction or some equivalent force is necessary to produce this motion. But, to meet him on his own supposition, let us admit that the elliptical motion of the earth is fully accounted for,—and then examine the circumstances which he supposes would have an influence on the descent of a body to the earth's surface.

In the first place, the resistance of the air will not have any tendency to force the body downwards. To remove all the circumstances that are not connected with the descent of the body,

let

let us suppose a ball to be dropped from the top of a high tower; in this case the air's resistance will retard the motion of the ball, instead of causing it to descend.

Secondly. The rotation of the earth cannot possibly deflect the ball downwards, because that, if the ball were acted upon by the rotary force only, it would fly off in the direction of a tangent to the earth's surface; consequently the rotary motion of the earth would have an opposite effect to that ascribed to it by Sir Richard.

Thirdly. That the annual motion of the earth cannot force the ball downward Sir Richard must know from the illustrations he has cited respecting the falling of bodies on board a ship in motion.

And, as none of these forces taken singly has a tendency to move the ball towards the earth's surface, it follows from the composition of motion that the joint action of these forces will not have any such tendency.

As to the angle Sir Richard has drawn as the measure of the deflective force, he might have made it any thing or nothing—just as was most convenient; consequently projectiles, if his reasoning be correct, would be subject to different laws in different parts of the earth at the same time, and at the same place to different laws at different times: but I do not find that he has made any experimental researches on this subject.

Sir Richard's anxious desire to make his hypothesis agree with the known phænomena of falling bodies has led him into a trifling geometrical error. The spaces described by the points C and F (see his figure, Phil. Mag., No. 230, p. 436,) in the same time will be as the circumferences of the circles they move in; and the circumferences of circles are as their radii, and not as the squares of their radii, as Sir Richard supposes.

Sir Richard is also incorrect in supposing that the effect of the rotary motion of the earth on falling bodies has not been considered: it was one of the strongest objections that were made against the Copernican system,—that if a stone were let fall from the top of a high tower, it would strike the ground considerably to the westward of the foot of the tower.

And as the experiments and reasonings of Galileo had not yet instructed men in the inertia of matter, nor in the composition of motion, the followers of Copernicus were unprovided with the true answer to this objection; viz. that the stone was a part of the earth, and therefore the annual and diurnal motions which were natural to the earth, were also natural to the stone; consequently the stone would retain the same motion with the tower, and strike the ground at the foot of it.

A more accurate investigation of the subject has led others to conclude, that the stone would fall a little to the eastward of the

 point

point over which it commenced its motion, in consequence of the velocity of rotation being greater at the top than at the bottom of the tower. The celebrated Laplace is said to have investigated this effect of the rotary motion of the earth, in the "*Bulletin des Sciences*," No. 75.

The Newtonian theory, on which the whole of physical astronomy is founded, asserts nothing more of gravitation, than that the result answers to the supposition, in every case, as far as observation reaches. Gravitation is not an occult quality, but a manifest property of matter, its truth appearing from the phænomena. And among these the attraction of mountains is a most direct and decisive proof that every particle of matter is endued with the power of attraction.

The effect of the mountain Chimboraço in Peru, on the plumb-line of the French philosophers; the experiments on the mountain Schehallien, by Dr. Maskelyne; the experiments at Marseilles, by Baron de Zach; and the interesting experiments of Mr. Cavendish*, are each of them an experimental proof that matter gravitates; and together form so complete and so consistent a body of experimental evidence, that, were the evidence derived from theory less perfect than it is, this would establish the truth of Newton's theory.

In a paper which indirectly accuses Newton of superstition,—which, in the idea of its author, will render it necessary to "*remodel*" his "*Principia*," and which professes to develop principles which will overturn the whole system of modern philosophy,—we certainly should expect to find something to correspond with these lofty pretensions, or at least something so plausible that we might admire even while we were obliged to condemn: but even in this its author has failed; he only shows that he is as imperfectly acquainted with his subject as he is with the subordinate sciences; that he knows little of the authors he pretends to refute, and still less of the system they have supported.

London, July 7, 1817. T. Tredgold.

IV. *New Outlines of Chemical Philosophy.* By Ezekiel Walker, *Esq. of Lynn, Norfolk.*

[Continued from vol. xlix. p. 354.]

The geometrician always defines the terms that he intends to use, before he begins to demonstrate a proposition; and the same rule ought to be observed in all physical investigations; for, if the meanings of the terms made use of be not understood, the investigations must be doubtful.

* Phil. Trans 1798.

According

According to the new theory, water consists of two principles, hydrogen and oxygen. Now before we begin to inquire into the truth of this theory, it will be necessary to understand the meanings of those terms. Dr. Henry observes that "every gas, it must be remembered, has at least two ingredients; the one gravitating matter, which, if separate, would *probably* exist in a solid or a liquid form; the other an extremely subtile fluid, termed *caloric*. In the example before us, *caloric* (and *perhaps electricity* and *light*) is a common ingredient both of hydrogen and oxygen gases; but the two differ in having different bases. The basis of the one is called hydrogen, of the other oxygen; and water may, therefore, be affirmed to be a compound, not of hydrogen and oxygen gases, but of hydrogen and oxygen *."

Dr. Murray observes that "the action of electricity affords a mode of resolving water into its constituent gases, and of combining those again so as to reproduce it †."

Now according to these statements, water is a compound of hydrogen and oxygen;—and hydrogen and oxygen are the component parts of water! This is nothing more than arguing in a circle; yet such is the basis on which is built the much celebrated fabric of the French doctrine of the composition of water.

As the component parts of water, according to the French hypothesis, consist of two ponderable *matters*, why are they not exhibited in a solid or a liquid form, divested of that supposed "extremely subtile fluid termed caloric?" But this, I believe, has never been effected; and therefore, till this be done, the existence of those *matters* can only be looked upon as an ingenious opinion, founded on conjecture.

If we were to reason from what we know, we might say that water is the basis of the two gases; but if we were to reason from principles the truth of which we do not know, we might then indeed conclude with M. Lavoisier and his associates, that the bases of the two gases in question are two unknown ponderable bodies called hydrogen and oxygen ‡.

We need only take a transient view of some of the grandest phænomena of Nature, to be convinced that the decomposition and recomposition of water are common operations. The water which falls from the clouds upon the surface of the earth is frequently converted into two invisible gases, by the two elements of combustion contained in the earth or upon its surface; and these gases ascending into the atmosphere become a part of it.

* Henry's Elements of Chemistry, vol. i. p. 206.

† Murray's Elements of Chemistry, vol. i. p. 304.

‡ Dr. Henry observes that "we have no knowledge of the properties of oxygen in a state of complete separation."—Henry's Chem. vol. i. p. 177.

When the two elements of combustion, thus carried up into the atmosphere, come into contact, thunder and lightning are produced; the light and heat thus generated fly off, and the water, which formed the bases of the two gases, is recomposed, and descends to the earth in a shower of hail, rain, or snow.

Now if we examine the following experiments on water, we shall find them exactly similar to those just mentioned; for the same undeviating law which takes place upon the surface of our globe, and in the atmosphere that surrounds it, obtains in the laboratory of the chemist.

When a Leyden jar is discharged a certain number of times into a drop of water, this fluid is wholly converted into two gases, which are equal in weight to the drop of water. Now, as nothing is present in this experiment, but water and the two elements which were contained in the jar, the two gases are compounds, consisting of those elements and water. Thermogen, the element of heat, converts a portion of the water into an invisible gas: photogen, the element of light, converts the other part of the water into another gas; water being the bases of the two aërial fluids. The two elements are kept separate by their bases; but an electric spark being passed through them, combustion is produced, and the bases of the two gases are resolved into a drop of water, of the same weight as the two gases; the two elements being imponderable. I think it would be wandering very far from that simplicity which is every where seen in the operations of Nature's laws, to suppose (for it can only be a supposition) that the bases of the two gases are not water, but two new matters; and when the gases are decomposed, these unknown *matters* are converted into water.

Lynn, June 30, 1817. EZEKIEL WALKER.

[To be continued.]

V. *Extract of a Letter from Colonel* MUDGE *to* WILLIAM BLACKWOOD, *Esq. relative to the Trigonometrical Survey**.

Edinburgh, June 7, 1817.

SIR, I HAVE the honour to inform you, that in consequence of the trigonometrical survey, carried on under my direction, having been brought on so far into the north as to admit of the description of the longest meridional line passing through Great Britain, M. Biot, under the authority of both the French and English Governments, is arrived in England for the purpose of doing, in the several parts of our arc, the same series of experiments that had been formerly done by himself and the Commission of the Board of Longitude, at Formentera, one of the Balearic Islands

* From the Edinburgh Monthly Magazine for June 1817.

in the Mediterranean, and other stations on the French meridian, proceeding from thence to Dunkirk.

The object of these experiments is, to ascertain the force of gravity at certain parts of our meridian, as connected with that of France and Spain. The pendulum is now erecting in Leith Fort, where every convenience offers itself for the experiment, and every wish has been anticipated by the chief engineer, Sir Howard Elphinstone. When the operations shall be completed, we propose to proceed to Kirkwall in the Orkneys, and near that place, or some more convenient situation, if any such can be found, we shall again set up the pendulum, and the ordnance zenith sector, the workmanship of the late celebrated Mr. Ramsden. Thus, while the experiments are carrying on to ascertain the force of gravity in that quarter, the observations will be made on proper stars near to the zenith, hereafter to be also observed, in finding the amplitude of the whole meridional arc. The base, now nearly completed in its measurement by Captain Thomas Colby of the Royal Engineers, in the vicinity of Aberdeen, will verify the sides of the triangles towards the northern part of our arc, connecting the Orkney Islands with the main land. It is probable that M. Biot and myself will leave this quarter for Inverness (where the ordnance sector is now deposited) about the end of this month; and we think it likely, if the weather should be fair, that our operations in the Orkneys will be finished early in August. When these observations shall be completed, we shall proceed to Yarmouth, on the coast of Norfolk, which lies nearly on the meridian of Formentera produced, and there we hope to be joined by M. Arago, member of the Institute of France, and one of the Commissioners of the Board of Longitude. By this co-operation, having accurately ascertained the latitude of this place, a notable addition will be made to the arc running south from Formentera to Dunkirk, independent of the great one, running north to the Orkneys; for we hope that the difference of longitude (being only a few degrees) will not have sufficient influence to interfere with the importance of this last connexion. We will repeat the experiments of the pendulum at Yarmouth, and afterwards proceed to Blackdown, near Weymouth, to the meridional limit of the English arc, where, having again observed the pendulum, and made observations with the zenith sector, on the same stars as are to be observed in the Orkneys, our united operations will close with Messrs. Biot and Arago erecting their clock at the Royal Observatory at Greenwich. It was to be always expected, that whenever peace should arrive, the science of France and England would affiliate, and by the united operations, in this particular, determine the magnitude and figure of the earth, by experiments carried on on a greater scale

scale than could be done individually, and with the utmost nicety and exactness. The whole arc from Formentera to the Orkneys will contain nearly 22° of the earth's meridian; and thence the quadrantal arc of the whole meridian, extending from the equator to the pole, being ascertained, will afford the best of all possible standards of length and capacity, whenever it shall be determined by the legislatures of both countries to equalize their weights and measures by the same common standard. The great arc deduced from these operations will be found to pass over a part of Spain, all France and Great Britain; Belgium has already followed the example of France, and has taken the standard from the same natural source. Thus, if by this participation, the three nations, from their united meridian, should agree to take the same standard derived from it, there seems little reason to doubt, the rest of the world, without loss of time or difficulty, would follow their example. W. MUDGE.

VI. *Experiments on Vegetation, tending to correct some erroneous Opinions entertained respecting the Effects of Vegetation on the Atmosphere. By Mr. J.* TATUM.

To Mr. Tilloch.

SIR, — THE opinion that the atmosphere is improved by *vegetation* has been supported by so many celebrated philosophers, for the last forty years, that few or none doubt its correctness. But in spite of authority, having long observed the very great analogy which exists between the animal and vegetable kingdoms in other respects, I could not but think that the anomaly respecting the effects supposed to be produced on the atmosphere by vegetation was incorrect; and that a wish to discover in Nature a method to reconvert the carbonic acid gas, liberated by animals, into oxygen, had betrayed the authors of this hypothesis into an error. I shall not occupy your pages in particularizing their various experiments, which even militated *against* their own doctrine; but beg to observe, that in general they were not conducted in a manner so natural and correct as to warrant the conclusions drawn from them. To ascertain the effects of vegetation on the atmosphere, I contend that the vegetables submitted to experiment ought not to be immersed in *pump or carbonated water*, nor *in a carbonated atmosphere*, as that is by no means the natural situation of plants, or indeed of any living body. To expect living bodies to perform their natural functions in unnatural situations is an absurdity; and to avoid this, I instituted a number of experiments which I thought more analogous to

to Nature, in order to determine what were the real effects of vegetation and vegetables on the atmosphere.

As germination is the first process of vegetation, I shall commence by calling your attention to the effects of that part of the physiology of vegetation on the air of the atmosphere.

Exp. I. For this purpose I placed a number of peas, barley, &c. to germinate in a given portion of atmospherical air in a glass receiver (the mouth of which was confined by mercury, in a groove turned in a slab of beech-wood. My reason for this method was to avoid the action of water on the air of the receiver, as well as to prevent a large portion of mercury being exposed to the same. The upper part of the receiver was furnished with a cock, to which I could attach a syringe, and draw out a portion of air to be examined, without disturbing the apparatus; to which also a funnel was occasionally attached, to supply water to the plant when necessary).

After a short time I found germination stop; but on lifting up the receiver so as to allow some air to escape and fresh air to enter, germination again commenced: this I repeated several times with similar results. Finding that germination ceased when seed was so confined, I had no doubt but that some alteration must have been produced on that fluid in which they had been inclosed. My next inquiry was to ascertain what this alteration was: for which purpose I agitated the air with lime-water. A considerable turbidness was the result; 1-13th was absorbed, and 4 inches of it with 2 in. of nitrous gas occupied 4·4 in.—but 4 in. of common air and 2 in. nitrous air occupied only 3·9 in.; from which we see that there was an abstraction of oxygen from the air of the receiver and a formation of carbonic acid gas,—most likely the oxygen of the atmosphere united with the carbon of the seed and produced the carbonic acid gas.

Exp. II. I placed a portion of barley to germinate in a similar manner; and when germination appeared to cease, I examined the air. To 2 in, I put 1 in. of nitrous gas, which occupied 3 in., so that no diminution whatever took place; consequently the whole of the oxygen had disappeared and formed some combination: at the same time 2 in. of atmospherical and 1 in. nitrous air occupied only 1·8 in.

Exp. III. August 10, 1816. Two small scarlet beans growing in a pot, and exposed to the *sunshine*, were bent under a receiver and confined by mercury. At the expiration of seven days 2 in. of the air and 1 in. of nitrous gas equalled 1·45 in.; but the above quantities of atmospherical air and nitrous gas equalled 1·42 in., consequently this process of vegetation had somewhat injured the air by abstracting its oxygen.

Exp. IV. June 4, 1816. An entire turf composed of Dutch clover

clover and grass (the area of which was 20 in.) was confined under a receiver (whose capacity was 150 in.) over mercury for three days, and occasionally watered through the cock at the top of the receiver. When the air was examined, 2 in. of it and one of nitrous equalled 2·3 in. But 2 in. of atmospherical and 1 in. of nitrous = 1·9 in. I have repeated these experiments at various periods, and have always obtained similar results.

Exp. V. Sept. 6, 1816. A dish containing a portion of stone-crop in a very healthy state, was placed under a receiver over mercury; and at the expiration of ten days I found 2 in. of the air and 1 in. of nitrous gas = 1·47 in., while the same proportions of common air and nitrous gas = 1·44 in.

Exp. VI. July 25, 1816. Several sprigs of bergamot mint growing in a pot were bent under a receiver as usual; and in six days I found 2 in. of the air and 1 in. of nitrous gas = 1·42 in. full; and 2 in. of common air and 1 in. of nitrous gas = 1·42 bare.

Perhaps it may be remarked, that the two last experiments produced but little effects on the atmosphere: but let it be recollected that the object of these experiments was to ascertain whether vegetation improved the air of the atmosphere, by imparting to it oxygen: and we see that in no instance whatever was the air of the atmosphere improved by vegetation; but on the contrary it was always somewhat injured, and in some instances the whole of the oxygen disappeared. Is it not fair then to conclude that, so far from vegetation improving the atmosphere, by decomposing the carbonic acid gas generated by animal respiration and combustion and liberating its oxygen, it like them combines with oxygen and generates the same kind of gas?

Having so far identified the physiological operations of the animal and vegetable kingdoms on the air of the atmosphere, I next tried the effects of factitious airs on plants, to see how far they might correspond with the effects of the same gases on the animal œconomy.

For this purpose I selected that plant which I could act upon in the most natural manner. Experiments VII. VIII. and IX. Three turfs of clover and grass were placed under receivers (as in the former experiments). The first was inclosed in nitrogen gas; the second in carbonic acid gas; and the third in atmospherical air (as a standard by which to compare the other two).

They were all placed in the open air, and exposed to the vicissitudes of day and night, sunshine and cloudy.

The effect of the nitrogen on the first turf was evident in one hour, by the leaves of the clover beginning to collapse and the leafstalks to bend; the leaves became yellow, and in three days

days the whole turf was completely dead, and when removed from the receiver possessed a very offensive and putrid smell.

The second turf, which was exposed to carbonic acid gas, betrayed signs of decay on the second day, similar to the above, but not so quick: on the fifth day this turf was completely dead.

The third turf, which was inclosed in atmospherical air during the above time, did not appear altered, except that the grass had grown considerably higher than when first introduced.

We have here further corroborating proofs of the agreement of the animal and vegetable kingdoms in the points under examination. We proved in our former experiments that vegetables, like animals, convert the oxygen of the atmosphere into carbonic acid gas; and in these latter experiments we find that those very gases which are fatal to animals are equally so to vegetables.

I could extend this paper to a much greater length, by selections from my Journal of the effects of fruits, flowers, new-cut grass, &c. on the atmosphere; in all of which the air of the atmosphere was much injured, and in most cases the whole of the oxygen was converted into carbonic acid gas in a few days. But fearing that I have already trespassed on the limits of your publication, I conclude,

Yours, &c.

Dorset-street, Salisbury-square, J. TATUM.
July 10, 1817.

VII. *Geological Queries to Mr.* WESTGARTH FORSTER, *Mr.* WINCH, *Mr.* FRYER, *&c. regarding the Basaltic and other Strata of Durham, Northumberland, &c. &c. By* A CORRESPONDENT.

To Mr. Tilloch.

SIR,—IT has given me sincere pleasure to observe at length, *Mr. Westgarth Forster*, becoming a Correspondent in your very useful Magazine;—I hope that in future he will become, like myself, *a constant reader* of your Work, and that he will often repeat his communications thereto, on Geological and Mining subjects. I beg to thank him for his attention, in p. 401 of your last volume, to two of my Queries, in p. 108 of your xlvth volume, and to request his early attention *, to several further

* I presume to hope and request, that some regular Subscriber to your Magazine, who may be in habits of intimacy with Mr. W. F., or who may live near to him, will early inform him of the request now made, and promote his reply, by the offer of the loan of their copy of your Work, for such purpose.

Queries

Queries, which I have taken the liberty of putting, in p. 12 of your xlviith volume, and pages 122 and 251 of your last volume; and that he will favour myself and many others of your Readers, to whom I know the same would be highly agreeable, with his full and explicit answers, to all such of these queries, as his local knowledge of the northern parts of England, may now, or hereafter enable him.

Particularly, as my 2d question intimates, as to *the fact*, whether or not, the "great whin sill" or stratum of Basalt (shown in p. 152 of his "Treatise on a Section of the Strata," &c. a very useful and cheap Work, printed and sold by Preston, of Newcastle) has not such *a continuous edge* on the surface, as clearly indicates it to form, like each of the other principal Strata, a vast *extended plane* (having curved parts), within the Earth, *conformably*, *with* its under-lieing and with its over-lieing strata: although *its great variation of thickness*, from eight fathoms to more than thirty fathoms (as is mentioned, p. 41 of the Treatise) may occasion its basset-range to assume, locally, *the appearance of detached and over-lieing masses of Basalt*, so as very closely to "resemble those of *the King's Park* at Edinburgh," as Mr. Winch has truly observed, in page 101 of your xlviith volume.

It seems therefore material I should mention here, that since Mr. Winch made this remark, the environs of Edinburgh have, for the first time I believe, been mapped by an experienced *Mineral Surveyor*, Mr. John Farey Sen., who is said to have minutely examined every part of the surface of the District; the immediate object of which Survey was, to ascertain the situations, extent and positions, of the *porous* and the *water-tight* Strata or Dykes, which supply or intercept *the springs of Water*, in the district around that City; and from which examination it results, as I am informed*, that "the Strata of the King's Park," are now divested, of all *the peculiarities* which, on the one hand certain Jamesonian Theorists, from the application of their Geognostic Dogmas to insufficient Observations, had inferred and said, as to the same consisting, of *unconformably over-lieing Basaltic masses*, as detached parts, of *the most recently formed or latest deposited Strata, of the district*; and on the other hand, what certain Playfairian

* Lately, in a Letter from a Friend in Edinburgh, who says, that a manuscript copy of such parts of the Report of Mr. Farey, as have been delivered to the Lord Provost and Corporation, which describe the Strata and relate to the Springs, is in private circulation there. It will remain now therefore to be seen, whether the Edinburgians, who hitherto have so readily and warmly entered into disputes on Geological *Theories*, will cause these *localized descriptions* of the principal *Strata*, and their very curious ranges and positions, in the vicinity of their City, *to be published*, and candidly examined: and whether they will in any way call for, and make the large *Mineral Map* known, from whence, as my Correspondent says, these descriptions

fairian Theorists, from Dogmas more wild and fanciful, and from equally or more superficial Examinations, had inferred and maintained, as to the King's Park mass, being *a heap of Lava*, ejected, in comparatively modern times, with regard to the ages of the Strata, from the adjacent crater of an extinct Volcano, which had broken up through those Strata!

And I doubt not but Mr. Forster and Mr. Winch, and many others of your Readers will be pleased to hear, that the application of those simple and almost self-evident principles, on which intelligent and *practical* Colliers and Miners are entirely agreed, throughout Britain, show incontestibly, that these Basaltic Strata, whose edges in Arthur's Seat Hill in the King's Park (close on the east side of Edinburgh) are now seen standing, locally, so much higher than elsewhere in the immediate vicinity, are the very same Strata, which form the south-eastern slope and highest parts, of the Pentland Range of Hills; and that these same Basaltic strata, regularly *under-lie the great Coal Trough*, situated to the south-east, east and north-east, presenting their edges all round, from underneath the same, not only in Edinburghshire, but across the Firth of Forth into Fifeshire: the principal Trough, making a turn therein, first NW then W, and then SW, through Clackmananshire, and again across the Forth, into Linlithgow and Stirling Counties, and thence towards Glasgow; which latter Coal-fields, heretofore thought by many Persons, to be separate and distinct ones; now, not only appear to join, by twice crossing the Forth, but the same Basaltic strata, everywhere appear rising from under the edges, of this complicated system of very crooked and branching *Troughs** in the strata, in which these Coal-fields lie; which principal Trough, sends off other branch

scriptions were taken; in order, to examine minutely into, and either acquiesce in, or confute and correct, the representations, therein made, by Mr. F.; or, whether the long-promised, and now, as it is said, *the forthcoming*, "Illustrations" of Mr. Playfair, and "Geognosy" of Mr. Jameson, will, in silence pass over these recent Observations; which seem, so strongly *to contradict each of the Theories*, which, almost every very modern Writer, has, untruly, and very improperly, said to be those, *in favour of one of which, every Geologist* is now *agreed!!*. By which unworthy artifice, so often and unblushingly played off, of late, the task of defending, each their own set of whimsical Dogmas, against the facts of Nature, and the published Observations of several Writers, is lessened, into that of confuting, another and equally or nearly as absurd a set of Dogmas, which has thus, by themselves, mutually, been conjured up into importance, for the mere purpose of obtaining an easy victory over it! each,—in the opinions of their own partizans.

* The term *Basin*, from its almost invariable application to something *circular*, or near to it, is very inapplicable to these local fields of particular Strata, and should cease to be used by Geologists, who aim at perspicuity and accuracy.

Troughs,

Troughs, through Haddingtonshire to the Coast south-east of Dunbar, and another through Fifeshire, to the Coast SE of St. Andrews.

It is perhaps not less important, that I should mention to Mr. Forster, regarding the other comparison which Mr. Winch has truly made, in the page already quoted, between the "Great Whin Sill" of Durham and Northumberland, and "the Toadstone of Derbyshire;" viz. that the facts ascertained thereon, in 1807 to 1811, by Mr. Farey, and confirmed by subsequent and more minute observations, made by Mr. Elias Hall, as is stated in vol. i. of Mr. F's Derbyshire Report, and in pages 113 to 115 of your xliid volume; these show, that instead of mere local "wedge-shaped beds of Basalt or Lava," as the late Mr. Whitehurst (deluded by the fanciful *Plutonic* Theory, which he was seeking to support) has in some parts of his "Inquiry" stated, to exist, underground, in the Peak Hundreds of Derbyshire, to which representation Mr. Winch seems here alluding; that on the contrary, the 1st or upper *Toadstone* or Basaltic Rock, to which this "great whin sill" seems undoubtedly referable, I think, is *a perfectly continuous stratum*, (although, in places, it is very unequally thick, as well as variable in substance) under-lieing the adjacent Coal-field, with the intervention of numerous beds of *Limestone* (of the 1st Rock, separated by numerous partings and wayboards of Clay), as is also the case (but with considerable variations in thicknesses, &c.) completely round, within the Basaltic border of the Lothian, Fife, Stirling, and Lanark, &c. Coal-fields, in the very extensive and complicated Troughs in the Strata, above-mentioned: as my Edinburgh Correspondent, alluded to in a former Note, has mentioned, from information he had derived, from Mr. Farey's recent researches and statements.

The concluding part of my 2d Query, in page 124 of the last volume, has in part been answered already by Mr. Forster, in p. 41 of his "Treatise," by his saying, that the "Great whin Sill," appears at Caldron-snout water-fall, on the Tees River: I shall however, be greatly obliged, by his stating in your work, if he can, all the requested particulars, regarding its dips there, &c.?; and also, that he will mention, all those particulars, as to the Strata above or below it, &c. which are visible in the upper part of the Tees valley, from whence he so confidently drew his conclusion, years ago, that this Basaltic mass in Teesdale, is part of *the same stratum*, which appears at Dufton-fell?.

I am sorry Mr. W. F. appears formerly to have paid such slight attention to the fossil *Shells*, in the Ironstone balls, in the Shales, and in the Limestone, &c. *interlaying the Coal-seams*; because, I can assure him, that these Shells, may be made the most important

portant *helps towards identifying the Strata*, where *their actual continuity*, or sufficient of *the series of Strata* being visible, are wanting, for so indicating identities; even, by those Observers, who, however well and usefully, they may *know Shells, by their appearances*, when carefully compared with each other, yet possess no technical or conchological knowledge, for enabling such persons, to *name* or *describe* Shells, in Language or in *Drawings*, which would be definite, or satisfactory, to general Naturalists, as was the case with *Mr. Wm. Smith*, the Mineral Surveyor, during many of the first years he was employed, in collecting and arranging, many hundred Shells, and other species of Organic Remains, each Specimen properly referred, to *its local seat and stratum*; which Specimens, now, that they are lodged in the *British Museum*, for the free use of the Public, others can, with the greatest facility and satisfaction, depict, name and describe, with all due technical accuracy.

I have mentioned thus much, in hopes of inducing Mr. Forster in future, to imitate Mr. Smith herein, as far as his opportunities of seeing fossil Shells may extend; and, in order to refer him to a Paper on this subject, which you did me the favour to insert at p. 274 of your xlvth volume: and particularly, to request his answers to my 3d head of Queries, already referred to.

It has given me pleasure, and I doubt not will do so to many of your Readers, to see, that Mr. Forster is able, so importantly to vindicate the character, for accuracy, of the *Section of the Strata*, which he published in the year 1809, as already mentioned, as to assert, that all the latter and lower parts of the same, were entirely made *from his own observations and admeasurements*, at several mining fields, and bassets of the strata: and I beg to remark, that Mr. F. would confer a further and lasting obligation, if he would send for insertion in your Magazine, an account of the steps which he took, whether by comparing the overlapping or repetition of his Strata, *measured in different Mines, Works, or Places*, or otherwise, for avoiding errors, *in joining these detached observations* together?: a point on which, I think I remember having read the expression of some doubts, particularly as to the junction of the Lead-Series and the Coal-series, in some former volume of your Work, but which at present I am unable, more particularly to quote.

Mr. Winch, Mr. Fryer, Mr. Buckland, &c. to whom my Queries referred to, were in the first instance more particularly addressed, will I hope and trust, excuse the reference *also*, of the same queries to Mr. Forster, so expressly as has now been done; and that the same, may not lessen the chances we had, of any answers thereon, from all or any of these Gentlemen, to whom—

Mr. Winch in particular, Geologists are already so deeply indebted, and from whom, still, so much is expected by many; in particular by, Sir,

Your humble servant,

July 12, 1817. A CONSTANT READER.

VIII. *Report of the Select Committee appointed to consider of the Means of preventing the Mischief of Explosion from happening on board Steam-Boats, to the Danger or Destruction of His Majesty's Subjects on board such Boats.*

YOUR Committee entered on the task assigned them, with a strong feeling of the inexpediency of legislative interference with the management of private concerns or property, further than the public safety should demand, and more especially with the exertions of that mechanical skill and ingenuity, in which the artists of this country are so pre-eminent, by which the labour of man has been greatly abridged, the manufactures of the country carried to an unrivalled perfection, and its commerce extended over the whole world.

Among these, it is impossible for a moment to overlook the introduction of steam as a most powerful agent, of almost universal application, and of such utility, that but for its assistance a very large portion of the workmen employed in an extensive mineral district of this kingdom would be deprived of their subsistence.

A reference to the evidence taken before your Committee, will also show with what advantage this power has lately been applied, in Great Britain, to propel vessels both of burthen and passage, how much more extensively it has been used in America, and of what further application it is certainly capable, if it may not be said to be even now anticipated in prospect.

Such considerations have rendered your Committee still more averse than when they entered on the inquiry, to propose to the House the adoption of any legislative measure, by which the science and ingenuity of our artists might even appear to be fettered or discouraged.

But they apprehend that a consideration of what is due to public safety has on several occasions established the principle, that where that safety may be endangered by ignorance, avarice or inattention, against which individuals are unable, either from the want of knowledge, or of the power, to protect themselves, it becomes the duty of Parliament to interpose.

In illustration of this principle, many instances might be given; the

the enactments respecting party-walls in building, the qualification of physicians, pilots, &c. the regulations respecting stage-coaches, &c. seem all to be grounded upon it. And your Committee are of opinion, that its operation may, with at least equal propriety, be extended to the present case, on account of the disastrous consequences likely to ensue from the explosion of the boiler of a steam-engine in a passage-vessel, and that the causes by which such accidents have generally been produced, have neither been discoverable by the skill nor controllable by the power of the passengers, even where they have been open to observation.

Your Committee find it to be the universal opinion of all persons conversant in such subjects, that steam-engines of some construction may be applied with perfect security, even to passage-vessels; and they generally agree, though with some exceptions, that those called High Pressure Engines may be safely used with the precaution of well constructed boilers, and properly adapted safety-valves; and further, a great majority of opinions lean to boilers of wrought iron or metal, in preference to cast iron.

Your Committee therefore, in consequence, have come to the following Resolutions; which they propose to the consideration of the House:

1. *Resolved*, That it appears to this Committee, from the evidence of several experienced engineers, examined before them, that the explosion in the steam packet at Norwich, was caused not only by the improper construction and materials of the boiler, but the safety-valve connected with it having been overloaded; by which the expansive force of the steam was raised to a degree of pressure, beyond that which the boiler was calculated to sustain.

2. *Resolved*, That it appears to this Committee, that in the instances of similar explosions, in steam-packets, manufactories, and other works where steam-engines were employed, these accidents were attributable to one or other of the causes above alluded to.

3. *Resolved*, That it is the opinion of this Committee, that, for the prevention of such accidents in future, the means are simple and easy, and not likely to be attended with any inconveniences to the proprietors of steam-packets, nor with any such additional expense as can either be injurious to the owners, or tend to prevent the increase of such establishments. The means which your Committee would recommend are comprised in the following regulations:

That all steam-packets carrying passengers for hire, should be registered at the port nearest to the place from or to which they proceed:

That all boilers belonging to the engines by which such vessels shall be worked, should be composed of wrought iron or copper:

That every boiler on board such steam-packet should, previous to the packet being used for the conveyance of passengers, be submitted to the inspection of a skilful engineer, or other person conversant with the subject, who should ascertain, by trial, the strength of such boiler, and should certify his opinion of its sufficient strength, and of the security with which it might be employed to the extent proposed:

That every such boiler should be provided with two sufficient safety-valves, one of which should be inaccessible to the engine-man, and the other accessible both to him and to the persons on board the packet:

That the inspector shall examine such safety-valves, and shall certify what is the pressure at which such safety-valves shall open, which pressure shall not exceed one-third of that by which the boiler has been proved, nor one sixth of that which by calculation it shall be reckoned able to sustain.

That a penalty should be inflicted on any person placing additional weight on either of the safety-valves.

4. *Resolved,* That the Chairman be directed to move the House, that leave be given to bring in a bill for enforcing such regulations as may be necessary for the better management of steam-packets, and for the security of His Majesty's subjects who may be passengers therein.

June 24, 1817.

Mr. Donkin's *Evidence.*

[Mr. Donkin's description of the construction of the boiler of the Norwich steam-boat was similar to that given in our former Numbers; we therefore omit it.]

Is it your opinion, that any boiler so constructed was unsafe? —As a high pressure boiler, certainly.

What do you call a high pressure ?—I should call from thirty pounds upwards high pressure; the technical phrase is applied to engines where the motive force is given by the expansive force of the steam.

Define what is the technical distinction between high pressure and low pressure engines ?—When water is made to boil in the boiler, and confined so as the steam is not allowed to make its escape, it continues to acquire expansive force as it receives increase of heat; in the high pressure engine, the piston of the steam cylinder is forced down by the expansive force of the steam alone, against the resistance of the atmosphere; when the piston has arrived at the bottom of the cylinder, a valve is opened,

opened, and the steam is allowed to escape into the atmosphere, and the operation is reversed; the piston of the cylinder is made to ascend by the same kind of force. In the condensing engine, or the low pressure engines, the steam having been once permitted to fill the cylinder, a communication is then made between the cylinder in which the piston works and the vessel in which the steam is condensed:—that is the distinguishing feature of the two engines. I will describe a further difference, which contingently arises out of the use of the two: that is, in the high pressure engines the engineer has it at his option to use what degree of expansive force he pleases, to convert an engine adapted for the power of five horses, or producing the power of five horses, to that of ten horses, or to any other extent which he may think his materials capable of sustaining. In the low pressure or condensing engines, the steam can never be advantangeously employed above from two and a half to six pounds upon a square inch.

Whatever power there is in what you call a high pressure engine, the pressure in that engine may increase the power beyond what it is calculated for, and by means of that may render it dangerous?—Certainly.

Is it your opinion, that a boiler could be made of proper materials, with safety-valves, and under proper guard and direction, to make that high pressure perfectly safe?—That would depend upon the quantity of pressure to be used; a safety-valve might be carried to three hundred, or to any assignable force. I think that a high pressure engine may be made safe to a certain extent, but where they are left *ad libitum* they never can be perfectly safe.

Do you mean to convey the idea, that it is impossible or difficult to adapt to a high pressure engine one or two safety-valves joined with a mercurial gauge, acting at the same point of pressure, so as to make it equally safe with that upon any other construction?—In answer to the first part of the question, relative to the safety-valve, I think I have answered that already; that we can apply a safety-valve to any degree of pressure without any difficulty, but that the safety of the engine does not totally depend upon the safety-valve.

State upon what other circumstances the safety of the engine depends?—My idea of the difficulty of obtaining a proper degree of strength at all times in the materials of which boilers may be made, arises from the constant deterioration which those boilers must be suffering from the action of the fire, and from the various degrees of expansion and contraction operating on different parts of the boiler.

Is it then your opinion, that in high pressure engines carried

to that extent you mention, that danger would always operate? —It would not always operate, but it would be extremely liable to accidents.

In fact, you yourself would not choose to use a high pressure engine, from the difficulty which exists, either more or less?—That is my opinion.

Have you made any calculation what would be the force required to be used to propel a boat in navigable rivers or canals? —This does not admit of a definitive answer. It depends entirely upon opinion, how far one force would be dangerous and another not; but if steam-engines are employed for the purpose of propelling boats, that may be effected with perfect safety by the low pressure or condensing engines, where the pressure need not exceed six pounds to the inch.

Of course that must depend upon the resistance to be made, and the velocity required for the boat?—Then I must make choice of a more or less powerful engine; I think it just to state to the Committee, that there is an advantage to be derived from the use of high pressure engines on board boats, which are necessarily loaded differently at different times; this different loading requires a different power in the steam-engine, and the high pressure engine is capable of having this additional power given to it without any difficulty, whereas in the low pressure engines they are confined to the force first assigned to them.

What is the maximum of the low pressure engines?—I scarcely ever saw them beyond six pounds.

In high pressure engines there is a great saving of fuel?—There is in one, a peculiar kind of those called high pressure engines; there is a considerable saving of fuel in Woolf's engines; but in the common ones, I believe there is but little saving.

If therefore the engines were to be used where the saving of fuel would be of considerable consequence, high pressure engines of a certain construction would be better adapted for that purpose than low pressure engines?—Where the saving was of much consequence.

If engines were to be used at sea, it would be of considerable consequence, the engine and the fuel being contained in a smaller compass?—Woolf's engine is not in a much smaller compass.

When you talk of the deterioration of the boiler, how long would a boiler, properly constructed and constantly used, be used with safety?—That is extremely uncertain; I have known one boiler worn out in six months, and another used for seven or fourteen years; the strength of cast-iron boilers is extremely uncertain; cast iron contracts in various degrees in different places, and therefore is liable to break.

You

You think that all cast-iron boilers are dangerous?—Certainly, when used for steam of high expansive force.

In your former answer, where you spoke of the extreme difficulty of so regulating high pressure engines as to insure their safety, did you mean to speak of those which had cast-iron boilers, or of both cast and wrought metal ones?—Chiefly as to the cast iron; it is more practicable to make a boiler of the malleable metals to resist a high pressure, as far as the tenacity of the metals is concerned; but another difficulty occurs, which prevents the application of the malleable metals to boilers for high pressure engines, which is that of rendering the joining of the plates secure.

Do you mean then to say, that wrought-iron boilers are not in frequent use to high pressure engines, in point of fact?—I believe they are in much less frequent use than the cast-iron boilers; and in Woolf's engine they are scarcely used at all.

Is not the cast-iron boiler much cheaper than the wrought?—I can scarcely tell that; I should think the cast iron would be cheaper, if made of equal strength.

In case of the explosion of a cast- or a wrought-iron boiler, which is attended with the greatest danger?—Cast iron, unquestionably.

Why?—From the frangible nature of the metal.

What do you apprehend to be the common effect, in case of the explosion of a cast-iron boiler?—The metal is broken into fragments, and driven off with great violence in various directions.

What is the effect when a wrought-iron boiler gives way?—Generally a rent; but I have seen one instance of a wrought-iron boiler, where the whole of the upper part was rent from the bottom, driven through the house in which it was placed, and carried to a considerable distance; I believe several yards.

Do you apprehend, that speaking generally, and unless by some extraordinary circumstance, such as the wilful shutting of the steam-valve, there would be any reason to apprehend such an effect as you have just now mentioned, to arise from the renting of a wrought-iron boiler?—No, I scarcely think it possible.

Supposing the boiler to be made of wrought iron, or copper riveted, and safety-valves properly adjusted, with a mercurial gauge also adapted in its diameter with due regard to the size of the boiler, do you conceive that any reasonable apprehension could arise respecting the safety of a high pressure engine?—I think there might; but with less apprehension as to the extent of the destructive effect to be produced.

You speak of less apprehension as to the destructive effect; have the goodness to explain that?—On account that in the

 malleable

malleable metals a simple rending generally takes place, it would seldom happen that the upper part of the boiler would be torn off; but in the cast iron the fragments would be scattered about, and be more destructive.

Do you not know, that wrought-iron boilers have been used to all sorts of steam-engines for a considerable time past?—Yes.

Did you ever hear of any other than the single instance that you have mentioned, in which a wrought-iron boiler burst in such a manner?—No.

Do you know what was the occasion of the top being blown off in the instance you mentioned?—We cannot tell what was the immediate reason, but I suspected it to arise from the shape of the boiler.

What was that shape?—The bottom was of the usual waggon-shape boiler, convex inwards; the concave part of the boiler was over the fire, and those who examined it with myself imagined that the engine-keeper had suffered the water to be expended, or the whole of it nearly evaporated, leaving a small portion of it in the lag of the boiler.

The boilers invented by Mr. Simms and Mr. Woolf were all of them cast iron?—I believe they were; I never knew them make any other.

Mr. Woolf's boiler has been in use nearly ten years?—I believe it has.

Did you ever hear of any accident happening to their boiler? —Yes, I have; I heard it stated the other day, by a brother-in-law of mine, Mr. Hall of Dartford, that he had known two or three accidents, but without any fatal or injurious effects.

How many engines of the high pressure character have been blown up?—I have heard of several.

Are there more than four?—A great many more, if there are taken into the account those which have exploded in America as well as here.

Do you consider low pressure boilers are safe from explosion in all instances?—Used with no further pressure than six pounds.

What renders them safe?—Because they never employ steam of high expansive force in them.

What are the means by which they are prevented from using steam of high expansive force in them?—Because it would be against the interest of the persons using them to employ it.

Is there any other guard against the condensing engine receiving such a charge of expansive steam as will burst it, than the care of the engineer or the interest of the owner?—Certainly none; because I have known instances where they have used in the same engine both steam of a high expansive force, and condensed it at the same time.

You

You have never heard of low pressure boilers blowing up?—No; I have never known of any explosion with injurious consequences; they give way repeatedly; but do no injury.

Is there any thing in low pressure boilers which may be depended upon absolutely, for preventing the steam acquiring an expansive force beyond what is intended?—Well regulated safety-valves; mercurial gauges or water gauges will at all times secure it.

Do those means of limiting the expansive force in low pressure boilers continue perfectly efficacious under all circumstances of misconstruction and mismanagement?—That entirely depends upon the construction; I have known safety-valves fail in their action from bad construction.

Can such or similar means be applied to high pressure boilers? Yes, certainly.

Is not the feeding of low pressure boilers usually done by a column of water; and is not this column the great reason of their safety?—That is one reason, but they ought to have a safety-valve besides.

Is it not the principal reason of their safety?—It is the most secure one.

If the feeding column of water be taken away, is not the security left to depend upon the safety-valve?—Unquestionably.

Are low pressure boilers employed in boats always or commonly fed by a column of water?—I never saw an instance of it.

If the mechanical means which are used to render the low pressure boilers secure succeed, will not similar means render high pressure boilers secure?—As far as the expansive force is not permitted to arrive beyond certain limits, so far it will afford security.

At what expansive force are low pressure boilers safe according to their usual construction?—I have seen very few boilers constructed for the purpose of a low pressure engine, or a condensing engine that would sustain a pressure of ten pounds, without occasioning considerable leakage, or without forcing the joints.

Are they not very often used with a force to render them unsafe?—I never knew an instance of it.

Is not the explosion of them likely to do mischief?—Not under the pressure they are capable of sustaining.

Not even if they are made of cast iron?—Certainly.

Are they uniformly made of wrought iron?—No; several of them are made of cast iron.

Are not the greater number of them made of cast iron?—No; I apprehend not.

You cannot then state to the Committee how many instances of explosion you know of high pressure boilers?—No, I cannot.

Are they more than in low pressure boilers?—I never heard of an explosion in the low pressure boiler of any consequence whatever, merely a giving way of the plates or the wearing out of the boilers; not such a bursting as can be called an explosion.

May not every instance of explosion of the high pressure boilers with which you are acquainted, be traced to bad construction, or palpable mismanagement?—I have never examined many of them, and therefore what they may be immediately traced to I do not know; but all the explosions I have heard of have been occasioned by the use of steam of high expansive force; the one I visited at Norwich certainly arose from the defective construction of the boiler: it was extremely ill constructed.

Was it not as well from the palpable mismanagement of the engineer?—That I do not know; we were told that it was; I have no doubt there had been very great temerity and rashness.

Was not that high pressure boiler which blew up in London the other day at a sugar-house, entirely owing to the most palpable misconstruction?—I saw the boiler after it had burst; and I certainly should not have made a boiler in that shape, to have withstood the pressure which it was intended to bear.

Was not that boiler made of a different thickness; one side of it three-quarters of an inch thick, and the other side two inches thick?—Those are very nearly the dimensions; but in addition to that, there was a defect in the casting, what we call a cold shut in the iron.

Is not the use of high pressure steam completely in its infancy?—Certainly, its introduction to general use is of much later date than the low pressure steam-engines.

It is in fact to be considered as an invention of recent date?—It is.

Have not material improvements taken place in the construction and use of high pressure boilers, in consequence of the accidents which have happened?—I conceive Woolf's mode of constructing boilers to be a considerable improvement; a very material one I have likewise been told, though I have never seen one, that Trevethick has invented; a method of making boilers by increasing their length and decreasing their diameter, so as to render them capable of sustaining pressure to a much greater degree than heretofore.

Have more accidents occurred since the invention of the high pressure boilers, than might have been expected from the invention of any new system whatever in the mechanical construction of engines?—Perhaps not.

What

What expansive force of steam is generally employed in those high pressure engines?—I fancy that is very variable, from 30 pounds to 120 upon the square inch, or even perhaps higher than that.

Instances have been known in which a boiler has been worked at 160 and 180; have there not?—I have heard of such things, but I never knew of them.

What is the proof to which high pressure boilers are generally exposed previous to their being used?—The most eligible proof they ought to be exposed to is by water.

To what pressure?—I should think double the pressure to which they are intended afterwards to be subjected.

What is the estimated force in your opinion, which would burst a high pressure boiler of the best construction?—That is very different, because it depends upon the strength and construction of the materials; I never entered into the calculation.

Have not the greatest advantages been derived to the mines and manufactures from the use of the high pressure boilers?—I believe inestimable advantages.

Have you any doubt that Cornwall has derived an advantage which may be considered as incalculable from them?—None in the world.

Do you think, from the few accidents which have occurred in the use of them, there is any better argument to be brought against the permitting them to be employed, than could be derived from the accidents which have arisen in the explosion of gunpowder in the clearing passages in the mines?—No, not as applied to the mines, certainly.

You have mentioned that security is afforded to the engine by feeding the boiler by a column of water; from what does that security arise?—The pressure from the steam in the boiler could never rise to a force greater than that which would be equal to the pressure of the column of water; whenever it did arrive at that pressure, or beyond that pressure, the water would be blown out and the steam would follow.

You have stated, that in the steam-vessels used upon rivers, this precaution is not resorted to?—I never saw one, and it would be extremely inconvenient.

For what reason?—On account of the undulations the water would be subject to; it would be thrown out of the pipe from the motion of the vessel; and other inconveniences would arise, such as bringing the pipe through the deck of the vessel.

Do you apprehend that a mercurial gauge would be exposed to the same inconveniences?—Certainly, I do; the altitude would be lessened by every new assumed position of the vessel; that is, if a tube placed vertically at first, should by the action

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of the vessel assume a diagonal or an oblique position, the altitude of the column would be lessened, and its consequent pressure upon the steam lessened.

Are you of opinion that there would be much difficulty in guarding against such an inconvenience as that?—Yes, I think there would, generally speaking; but a well constructed safety-valve would answer all the purposes.

Are not the safety-valves applied to the low pressure engines, even when the column of water is used to supply the boiler?—Most frequently they are; I have seen some without.

Did you never hear of the pressure in a condensing engine being raised by mismanagement as high as 19 or 20 pounds per inch?—No; I do not recollect that I ever met with such a circumstance; I have no doubt that it has taken place.

If such a circumstance may take place with a low pressure engine, do not you think that, according to the general calculation of the strength of their boilers to resist the usual pressure to which they are subjected, more danger would arise than in almost any case which could happen to a high pressure engine with a boiler properly adapted?—No, decidedly not; according to the general construction of low pressure boilers, they are so riveted together to withstand the low pressure they are intended to bear, and they always give indications of an increase of pressure long before I should apprehend any danger from it; I mean by the joints giving way, and the steam forcing a passage through them.

Do you mean to apply that to the cast-iron boilers?—No, certainly not; to the wrought-iron or copper boilers.

The question before put was meant to apply to a low pressure engine, fitted up with a cast-iron boiler? — As applied to the cast-iron boiler, I should say, that being constructed to bear a less degree of expansive force, an explosion would sooner take place, and therefore would be less dangerous.

Less dangerous in comparison with what?—With a high pressure boiler.

Do you mean to say, that an explosion of the cast-iron boiler of a low pressure engine which should be burst by an improper degree of pressure, would be less dangerous than the rending of a wrought-iron boiler, occasioned by a much higher degree of pressure?—I gave my answer as connected with the former question, with regard to the liability to danger from low pressure boilers; I take for granted, that if a boiler is constructed to be applied to a low pressure engine, that a commensurate strength will be applied to the materials of the boiler, and that in the case of applying a boiler to a high pressure engine, an adequate strength must be used there to the pressure intended; therefore,

if

if the low pressure boiler by any accident should be exploded, generally speaking, boilers made of the malleable metals must be much safer on an explosion taking place, at least they are not calculated to do so much mischief as the cast-iron boilers.

Have you made any experiments, or are you acquainted accurately with the effect of such as have been made upon the different quantities of fuel consumed in the high and the low pressure engines, in proportion to the quantity of power produced?—I have witnessed several experiments on Woolf's engines, where the object was to ascertain the comparative expenditure of coals or fuel in grinding corn between his engines and the low pressure or condensing engines, and the results were decidedly in favour of Mr. Woolf's engines.

You cannot speak as to the high pressure engines commonly in use?—I apprehend there is no saving of fuel, or very little; there may be a little.

What was the saving of fuel by Woolf's engine, as compared with the other?—The average effect in one case was the grinding eighteen bushels of wheat with one bushel of coals; the other average effect of Bolton and Watt's engine, on the low pressure engines, is the grinding of from ten to twelve bushels of wheat with a bushel of coals.

Do you know whether the power of the engines in lifting water, may fairly be reckoned at the same proportionable difference?—Yes; I believe they may. I do not speak from experiments; but I have no doubt as to the effect; by the reports from Cornwall, I am led to suppose it may be much greater.

Have you seen any account of the explosion of the steam-engine on board a boat in America, within a few weeks past?—No; I have not. I understand there has been one.

From any information you received at Norwich, did you hear of any conduct of the manager of that boat, which occasioned the explosion of the boiler?—No; the information I did receive upon that subject, was since we returned to London.

Mr. Thomas Lean's *Evidence.*

Will you state your profession, and place of abode?—I reside at Crowan in Cornwall, and I am employed by nearly the whole of the miners in Cornwall to inspect their engines, and make monthly reports of the work they perform.

You are then well acquainted with steam-engines of every various construction?—Certainly I am; I see fifty-seven every month.

Do you conceive that there is any material difference in the respective safety of those engines?—Some of the engines are certainly safer than others.

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Be so good as to state which, and why?—I conceive there is no danger whatever in the use of high pressure steam-engines; and for this reason, that in general, for an engine that is intended to be worked with high steam, the materials are made stronger in proportion than the materials used for steam of low pressure.

What are the precautions which you think it necessary to take, in order to render a high steam-engine perfectly safe from accident?—The materials should be made strong enough, and there is no difficulty in doing that; and there is a good deal depending on the construction of the safety-valve, which should be so constructed as to go quite easy and without any possibility of sticking.

Do you not think it of importance, if not necessary, that a boiler should have two safety-valves?—Certainly; and every high pressure steam-engine that I attend to has two safety-valves.

Do not you confine one of those from the engine-man?—Not in any instance.

Should you or not think it necessary, on board a boat for passengers worked by a steam-engine, that there should be an additional safety-valve to the boilers which the engine-man could not come at to prevent its operation?—That would certainly be very desirable, and I should think necessary.

Have you any choice, in point of safety only, between a boiler constructed of cast iron or of wrought iron?—Were I to have a boiler where I wished to have the greatest strength, I would certainly have it made of cast iron; I have not one doubt that a cast-iron boiler can be made much stronger than it is possible to make a wrought iron one; in fact, the explosions that we have had in Cornwall have all been in wrought-iron boilers, but I never had one in cast-iron boilers, nor have we had an accident from high pressure steam; all the accidents have been from low pressure steam in Cornwall.

To what do you attribute that?—I attribute that to the boilers not having their proportionate strength to the weight they ought to bear, that the high pressure steam-engines have.

Of what nature are those failures which usually happen in the wrought iron boilers?—The one which I witnessed the explosion of, threw off the man-hole door.

Do you mean that the bolts by which the man-hole door was secured, gave way?—Yes.

Are there not man-holes to cast-iron boilers?—There are.

Then might not the best constructed and the strongest cast-iron boilers have been equally liable to the accident you have been mentioning, from the mere failure of the bolts, by which the man-hole door was secured?—Certainly not, and for this reason,

reason, the man-hole door to a cast-iron boiler is contrived to be on the inside; it does not depend upon bolts at all as they are constructed with us, it bears against the side of the boiler.

Would it not be equally easy to affix man-holes so constructed to wrought-iron boilers?—There is no difficulty in doing it, either one way or the other.

Supposing a cast-iron boiler and a wrought-iron boiler to be exploded by having too great a pressure applied, from which of the explosions should you apprehend the greatest danger?—I think the danger is equal from one as the other.

In what manner do you apprehend then, that a cast-iron boiler would explode?—Probaby there might be some parts of the cast-iron boiler separate; and the wrought-iron boiler would probably rend.

Should you not then apprehend a greater danger from the explosion of a boiler which burst into fragments, than from one which only rent?—In every boiler that is built, there is one part of it weaker than another, and it is hardly possible for a boiler to be thrown about in fragments to do mischief. I should not feel any hesitation to sit on the cast-iron boilers I have seen in Cornwall when an explosion took place; I am convinced the explosion would take place at the under part.

Do you think it necessary or advantageous that those boilers should be proved at their first erection, and that that proof should afterwards be repeated at intervals?—It is certainly desirable it should be done at the first erection; they ought always to be proved; the cast-iron boilers which have come under my notice in Cornwall, I calculate to be sufficient to resist at least thirteen times the pressure of steam we have ever used in them.

To what heat are those boilers usually proved?—We work in general forty pounds to an inch in the high pressure boilers, and we prove them sometimes as high as three hundred.

By a proof of this nature, so much within the supposed capacity of resistance of a boiler, you do not apprehend that any risk is incurred of injuring it?—Certainly not.

And you yet conceive, that the proof is so far beyond the ordinary resistance which is required from the boiler, as that there is no danger whatever of its bursting with a pressure of forty or fifty pounds an inch, when it has been proved by a pressure of three hundred?—Certainly not.

Do you apprehend, that it is perfectly easy so to construct and to secure your safety-valves, as that no engine-man, however careless, shall be able to raise the steam beyond the pressure of forty or fifty pounds per inch?—There certainly is not the least difficulty in it.

You apprehend then, that with a boiler so constructed, so proved,

proved, and guarded against carelessness, there would be no danger whatever in any situation?—Certainly not; neither in a steam-boat or an engine employed in a manufactory or mines, or in any manner whatever.

As to the œconomy in the use of coals, what is your opinion? —My opinion is, that the high pressure engines in Cornwall have saved at least two-fifths of the whole consumption of coals in the county; in some instances it has saved three-fifths.

What means have you of ascertaining that fact?—In the pursuance of my ordinary employment, I attend to the various engines in Cornwall, and compute their duty; the quantity of coals that is consumed by the engines is rendered to me on oath; it is the same that is sworn to at the Custom-house. The ascertaining the weights which the engine lifts is carefully and correctly measured; and from this I calculate the work performed by the engines, of which I make a monthly report, and find, that those engines which work with a high pressure steam are more œconomical in their operations than those of the low pressure, so much so, that were the low pressure steam engines to be introduced into the mines of Cornwall, it would stop upwards of two-thirds of them.

Is the paper which you have, one of those accounts?—It is the account for the last month.

[It was delivered in, and read;—*Extracts from these Reports are given regularly in the* Phil. Mag.]

And this account you declare, upon your own knowledge, to be accurate as to the particulars it contains?—I do.

Do you consider it as important to the safety of an engine, that the boiler should be frequently cleansed?—If a boiler is foul, if there is a quantity of mud in it, it may prevent the water from coming in contact with the iron, and in that case the boiler is liable to injury; I have known a wrought-iron boiler to burst from that very cause; I never knew a cast-iron boiler to explode in any instance.

Is there any difficulty in subjecting the boiler to the usual proof, every time it is cleansed?—There is no difficulty whatever, any other than having the apparatus prepared for it, which is very easily done.

Is that apparatus either expensive, or difficult of construction, or of application?—No.

Can it be applied with ease by any engine-man or engine proprietor, who is at all acquainted with the construction or working of a steam-engine?—Yes; and the management of it is so plain that no person can misunderstand it, if they are unacquainted with all the other parts of the engine.

In what does this proof consist, and how is it performed?—

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The proof consists of first filling the boiler with water, and then loading the safety-valve to any point required; then injecting water by a forcing pump, till the safety-valve, with the additional weight upon it, is raised.

Have you any other suggestions to make on the subject of the safety of steam-engines, besides what you have already said?—I think not.

IX. *Notices respecting New Books.*

An Inquiry into the progressive Colonization of the Earth, and the Origin of Nations; illustrated by a Map of the Geography of Ecclesiastical and Ancient Civil History. By T. Heming, *of Magdalen Hall, Oxon.*

We have read this work with attention, and examined the large map, with which it is accompanied, with some degree of care. The whole exhibits much patient, and, when the nature of the inquiry is considered, we may add successful investigation.

The title of the work expresses sufficiently its object. However serviceable detached "scraps of chorography," embodied under the name of "an atlas," may be to those who have already attained proficiency in the science, there is great inconvenience in being obliged, while reading, to turn from one detached survey to another, and so to combine them as to obtain satisfaction. To obviate this, "and to facilitate by the most approved mode the acquirement of correct ideas, regarding the circulation of human societies through the remotest periods, it was designed to compass, in a general map, the whole scope of territory connected with the sacred, civil, and profane writings of antiquity, on such a competent scale as appeared sufficient for every requisite illustration, from the first colonial migrations of mankind, to the rise of the present nations of the earth, and still to confine the same within such a dimension, as might render it convenient for the most ordinary and general application and reference."

But the author had first to settle his point of departure—the second cradle of the human race. For this purpose the traditions, for they deserve not the name of records, of the Egyptians, the Assyrians, the Chinese, the Phœnicians, the Scythians, the Indians, the Persians, and Arabians; and the writings of Homer, Hesiod, Thales, Pythagoras, Plato, Hecatæus, Berosus, Abydenus, Alexander Polyhistor, Demetrius, Diodorus Siculus, &c. are examined, and compared with the writings of the Jewish law-giver. This subject occupies the author's first chapter, which he concludes with the following deductions:

 "First—

"First—That there is not to be found, in all the rival monuments of antiquity, any authority equivalent to, or that can in the least degree invalidate, the memorial of Moses.

"Secondly—That his writings are of so pre-eminent and extraordinary a quality, that the greatest efforts of human subtlety and art seem to have been often ineffectually exerted to counterfeit and nullify them.

"Thirdly—That the most profound sages—the most conceited theorists—the most celebrated historians—the most romantic poets and discursive geniuses of every pagan age and country seem to have resorted to his pages for information, and to have borrowed thence their only true notions regarding the primitive affairs of the earth; and that what they have feigned to deny as infidels, theorists, and enthusiasts, they have involuntarily admired and espoused as historians, critics, and philosophers.

"Fourthly—That the Pentateuch seems ever to have been the only source of faithful intelligence respecting the formation of the earth, and the rise of human society; and which its most illiberal and malevolent adversaries directly or indirectly authenticate.

"And lastly—That being, as it appears to be, unanimously attested by the whole world as the paramount evidence of the renovation of mankind after the flood, and of the first dispersion into colonies, it establishes for us those facts which no other volume in the world contains, and from which the history of the present population and political cantonments of the earth must necessarily be derived."

This leads the author to another inquiry. The testimony of Moses being found more consistent and satisfactory than any documents that have been compared with it; how come the moderns so far to disregard his authority as to place Ararat, where the ark rested, in Armenia, almost due *north* of Shinar? Moses says expressly, that the builders of Babel "journeyed *from the east.*" Where then should the Ararat of Moses be sought for? To this inquiry the author devotes the whole of his second chapter, which we shall quote entire.

"*Inquiry concerning the Place of the Mountains of Ararat.*

"'And the ark rested in the seventh month, on the seventeenth day of the month, upon the mountains of Ararat.' Gen. viii. 4.

"'And it came to pass, as they journeyed from the east, that they found a plain in the land of Shinar, and they dwelt there.'

"But before we proceed to the peak of Ararat, or the summit of Babel, to mark therefrom the overspreading of the earth by the posterity of Noah, we must endeavour to decide the geographical

graphical position of the former. With respect to the latter, it is required to be understood that it is the point uniformly alluded to when speaking of the plains of Shinar in the future parts of this inquiry.

"With regard to the situation of Ararat, even many of the pious fathers seem to have paid too much attention to legendary tradition, and too little to the pure fact: for it is certainly not reconcileable to good faith in Moses to say, that Mount Ararat, where the ark rested, is north, or north-bearing-west, of Shinar, when he has so explicitly said, that the people came thither from the east: and how learned and orthodox commentators could ever have been persuaded to adopt the mountain called Ararat, in Armenia, as 'the landing place,' is very unaccountable, as there is nothing but *the name* and *traditionary report* to authorize such a conjecture; and this quite contrary to the express words of Moses. That Ararat was eastward of Shinar, as the divine historian hath told us, there are many circumstances to show; but there can be no true judgement without evidence: therefore we will proceed to examine the authorities on both sides of the question.

"Epiphanius, Basil, Jerome, Eusebius, Berosus, Josephus, Nicholas Damascenus, and more, mention *reports* that part of the ark was to be seen in their times on the Gordiæan mountains which are in the south of Armenia: and the last-mentioned of them says 'that there is a mountain in Armenia called Baris, which in the Coptic language signifies *a ship*, 'whither,' as *the tradition* goes, 'some persons escaped in an ark, from the great flood; and that pieces of the wood were there seen for many ages after.'

"Now the positive testimony of either of these men would have been weighty; but the *reports* which they have listened to are nothing more than fume.

"Elmasinus says, 'he went up Mount Gordus and viewed the place where the ark rested,' but does not say he saw the ark there.

"There are other similar accounts in Bochart, Josephus, Wells, &c. but they are all equally superficial and unsatisfactory.

"Herbert says, 'that the highest mountain in Armenia is called Baris;' which he imagines is also called Damoan—'that it is between Armenia and Media—that he and his company rode up to the top, whence they had a prospect of the Caspian Sea, though 160 miles off—that there are numbers of Jews about the village of Damoan, at the foot of the mountain, who say they are the offspring of those transported thither by Salmonassar, 2 Kings xvii. 6—that they have never changed their seats, and that they have a constant tradition that the ark rested upon the mountain.'

mountain.' Herbert is here speaking as though he thought these Jews really knew something about the matter, when they must be as ignorant on the subject as the people of Del Fuego: for whether they belonged to the race of Jews carried off by Salmonassar, or not, it is just the same, as it was 1600 years after the flood that the Assyrian king transported his captives; so that, even of traditions, none could be more flimsy—how should strangers who knew nothing of the country for 1600 years after the event get hold of their tradition?

"Sir John Chardin informs us that Ararat lies twelve leagues east of Erivan. He considers it the same as the Gordiæan Mountains. 'The Armenians,' says this traveller, 'have *a tradition* that the ark is *still* on the top of it—the mountain is totally destitute of inhabitants, and perpetually covered half-way from the top with snow.'

"Struys, another traveller thither, is more minute in his account of Ararat. After a description of the stone and minerals of the rock, he tells us, 'that he went up the mountain to cure a hermit who was secluded there, of a rupture—that it is surrounded by several rows of clouds, the first of which is dark and thick; the next extremely cold, and full of snow; and the third so intensely cold that he was scarcely able to endure it—that above this uppermost stratum, and where the hermit's cell was, the air was quite mild and temperate—and the recluse declared to him, that 'he had neither felt a breeze of wind nor a drop of rain for twenty-five years, which was the time he had lived upon the rock'—that he further told him, 'that the air on the top was much more calm than where he resided—that it was not subject to change—and that, therefore, the ark continued undecayed'—that he obtained from the hermit a piece of wood of a brownish-red colour; and a piece of the rock on which he alleged 'that the ark rested;' in attestation of which he gave Struys a certificate to the following effect:

"'*Certificate.*—I with mine own hand cut off from the ark the piece of wood made in the form of a cross; and broke off from the rock, on which the ark rested, that same piece of stone.' (Signed) 'DOMINICUS ALEXANDER ROMANUS.

'Dated Mount Ararat, July 22, 1670.'

"Struys also informs us, 'that he was seven days travelling from Erivan to this mountain;' and 'that it is an entire rock without earth, trees, or verdure upon it.' He has given us a map of the Caspian Sea, from which it appears that Ararat is towards the western coast of that sea, north of the river Kir, and somewhere about the southern extremity of Caucasus; being about 300 miles east-bearing-northward from Erivan.

"Thevenot, and other travellers, bring us also *reports;* but vary

vary in the position of this mountain; so that, if any one of them is right, all the others are wrong: and every thing we are able to gain from these authors is, in the end, what Moses has briefly informed us; namely—that Ararat was the resting-place of the ark.

" What in the world could have possessed Mr. Struys? Surely it was very tame of such an enterprising traveller to turn back—after having surmounted the regions of clouds, and finding himself in such a serene climate, not to have visited the stupendous hulk! especially as he had such good surety of its being there, and in such excellent repair—not to have explored every corner of that mighty carrack, moored so high, which had once contained such an inestimable cargo—not to have followed up the grand effort, and have pacified for ever the eager solicitude which must still hang about this interesting mystery—to come away satisfied, after climbing so high, with that bit of splinter—and, that piece of stone!

" Wells has inserted Ararat in his maps almost duly north of Babylon, and nearly sixty miles westward of Erivan; but I have no idea upon what authorities.

" Cellarius says, that most interpreters take the Gordiæan mountains to be Ararat; and which are either a part of Taurus, or near it. In the Targum of Onkelos the mountains of Ararat are translated the mountains of Cardu; and in the Targum of Jonathan they are rendered the mountains of Kadrun.

" Many of the other commentators, whose notions are confined to Armenia, extend the interpretation, and say, the mountains of Ararat—the Gordiæan mountains—the Armenian mountains—using the plural, as we find it in Genesis, without pretending to fix upon any particular tor. But Moses did not speak obscurely, nor is it to be allowed that he spoke insignificantly, when he said 'they journeyed from the east:' therefore, to be ferreting about Armenia, for the sake of a string of contradictory rumours, is tantamount to a dereliction of faith, and a gross absurdity; because it is following rumour rather than fact: and it is pretty certain, that rumour can never cause the sun to rise in the north, nor the magnet to quit its old propensity. Indeed it is almost past supposition, that rumour should have withdrawn so many, from a point so plain and positive. What is categorically announced should be literally interpreted:—let us, therefore, try the fact against the rumour.

" In the first place, it is far from unlikely that Ararat is a primitive word, which generated out of the particular circumstance to which it refers; as the opinions respecting its precise etymon and signification are as vague and inconclusive as about its place. We must notice, that Moses applies it extensively,

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and not locally, by using it in the plural. This word may have been afterwards transferred as a name, applicable to Armenia, without the slightest reference to the ark: for in the space of from 700 to 900 years, which elapsed from the time of Moses to the ages of Isaiah and Jeremiah, great changes in countries must have taken place; and in those early establishments, nothing was long durable. As to names, they were the most fickle parts belonging to countries; for a name was easily carried from place to place, though a territory could not; so that, analogy of name, though found in Scripture, is no demonstration of identity; and Isaiah and Jeremiah allude to very foreign matters, in their mention of Ararat, to what Moses did. Indeed we might as well look for Damascus in the Desert of Arabia, as for the ark in Armenia; for the land of Uz is in the Arabian Desert, and Damascus is in the land of Uz: but we know that Damascus is not in Arabia; and therefore, we reason, that these must be two distant countries named alike.

"Now had the two great prophets spoken counter to Moses, it would have been much more melancholy and awful; and which they would certainly have done, if they had said that the ark grounded in Armenia: but, they neither wrote to conduct us to the ark, nor to lure us into any contrary pursuit; and we must here endeavour to persuade ourselves, that Ararat on the north, is not Ararat on the east, of Shinar; and that there is no contradiction between Moses and the two prophets; because, one event preceded the other nearly 1700 years; and because, the incidents were as foreign from one another, as they were distant in time.

"In our endeavour, then, to arrive at the truth, we cannot do better than retrace the geographical rhumb, which Moses has laid down for us, from Ararat to Shinar. In our progress along this track, from the position of the latter place, we come to that long and elevated range of mountains which some of the ancient writers have considered to be a continuation of Taurus and Caucasus; and which extend, according to Quintus Curtius, in an eastern direction all through Asia, even to the coast of China. From this grand ridge, several collateral branches stretch, from different points, towards the north and towards the south, and at the western extremity of which are the Gordiæan mountains of Armenia, part of which is supposed, by some of the authors we have mentioned, to be the Ararat where Noah alighted after the flood: so that, the resting-place of the ark may yet have been on these same mountains, though not in Armenia.

"Procopius says, that the Macedonians called the part of these mountains, on the eastern frontier of Persia, which had been previously called Paropamisus, by the name of Caucasus, in compliment

pliment to the victories which their hero Alexander won in those parts of the world.

"From this it has been erroneously imagined that the mountains of Taurus and Caucasus form a junction towards the south-west of the Caspian Sea, and continue on to India; and hence some have said that the ark rested on Taurus, and others have told us that it rested on Caucasus: but Caucasus commences on the north-eastern part of the Euxine, and proceeds in a rather south-eastern course, to the west shore of the Caspian, near to the mouth of the river Kir, where it ceases: and the heights of Taurus rise in the west of Asia Minor, and afterwards strike into two branches; one of which terminates at the river Euphrates, and the other, running north-east, ceases at the eastern side of the Euxine—therefore, if the declaration of Moses is to be verified, these mountains of Taurus and Caucasus have nothing to do with Noah and the ark; and what we find to have been falsely called Caucasus, we must, according to the information of Procopius, consider to be Paropamisus.

"It has been alleged by Buno, that these mountains of Persia are so high that the sun shines upon the tops of them during a third part of the night.

"It is remarked by the Holstein ambassadors, who visited Persia about two centuries ago under Brugman, that Curtius is not altogether wrong in saying that these mountains extend all through Asia; 'because the heights of Ararat and Taurus so nearly join them,' say these ambassadors, 'that they appear to be one concatenation of mountains.'

"Wilson's Asiatic Researches record some traditions of the Indians respecting the antediluvian ages; the flood; and the preservation of the remnant of mankind. He says, 'there is a mountain in the province of Candahar, that is called Aryavart, or Aryawart; on which, the tradition of that country says, the ark lodged.'

"This is a part of the ancient Aria or Ariana, (a very extensive country in the east of Persia, in the earlier ages:) and hereabouts we find several dialects remaining, of the Targum translations of Ararat, before mentioned, attached to different parts of the country; as Candau, Candu, Gaur, Goura, Gor, Gorgian, &c. Here also, besides Aria, Ariana, and Aryavart, are Herat, or Harat, Arsarath, Yerac, Herac, &c. And we may further remark, that in the Persian and Indian vocabulary the termination *at* is very frequent; as Amadabat, Surat, Guzerat, Gehan-abat, Estarabat, &c.

"In so obscure a matter we must lay hold of every little light; but, were there not evidences stronger than these, our attempt would be to no purpose. When a stream becomes so clogged and

and choked as this is, there is scarcely a possibility of delving through all the obstacles with which the versatility of time, the roots of prejudice and error, and the fashions and corruptions of language have conspired to fill it: and though it may be possible to remove some of the obstructions collected about its source, so as to get it to trickle, yet shall we never be able to come exactly to the fountain-head; and it would be a useless and unprofitable appropriation of time to attempt it, since what is impossible cannot be. But, very fortunately for us, it happens that such nicety is by no means indispensable to the success of the argument we are upon; which requires only, that we should deduce no judgement but what is conformable to the declaration of Moses; and that, subjecting ourselves to this restriction throughout, we should endeavour to work our way, as near as the circumstances will allow, towards the truth."

[To be continued.]

Mr. Accum has in the press, "Chemical Amusement," comprising a Series of curious and instructive Experiments in Chemistry, which are easily performed and unattended by danger.

Mr. Newman, Soho-Square, has just published a work entitled "Chromatics; or, An Essay on the Analogy and Harmony of Colours."

Speedily will be published, in one volume octavo, A Practical Inquiry into the Causes of the frequent Failure of the Operations of extracting and depressing the Cataract; and the Description of a new and improved Series of Operations, by the Practice of which most of these Causes of Failure may be avoided. Illustrated by Tables of the comparative Success of the old and new Operations. By Sir William Adams.

The first number of a new periodical work, entitled "Journal of the Academy of Natural Sciences of Philadelphia," has just reached this country from America. It contains, 1. Description of six new species of the genus *Firola*, from the Mediterranean, by MM. Le Sueur and Peron; with a plate. 2. An account of the new mountain-sheep, *Ovis montana*, by Mr. George Ord; with a wood-engraving of the horn of the animal. 3. A description of seven American water and land Shells, by Mr. Tho. Say. The work ends with an invitation to naturalists to make use of the Journal as a medium of communicating science.

It is expected and hoped that Mr. Abernethy will publish his very excellent observations on the discoveries of the late celebrated John Hunter in comparative and human Anatomy, delivered

livered at the College of Surgeons during his Lectures.—This gentleman has shown that we are in reality indebted to Hunter for many facts in natural history, &c. plagiarized by the modern writers on physiology. The publication of his Introductory Lectures, relating to Hunter's Theory of Life, &c. &c. were omitted to be mentioned in the Philosophical Magazine. They contained a sort of summary of the physiological opinions of that acute and truly philosophical reasoner. Among other things which the author has ably handled, we may reckon his remarks on the vital principle as some of the best,—not because any theory of life is therein established on demonstrative evidence, and placed beyond all controversy—for the obscurity of the subject renders this impossible—but because on a subject *in itself purely theoretical* he has followed a course of reasoning *founded* on the observance, and strictly philosophical throughout, and which is more consistent with the common sense of the thinking part of mankind in all ages past, as well as with popular feeling, than any other modern theory of life, or philosophy of mind. It is in this respect eminently contrasted to that confused farrago of scarcely intelligible words in which some modern writers have attempted, in humble imitation of the French school of philosophy, to convey and establish the gloomy and misanthropic doctrine of materialism, and thus confound the distinction of automatic and animal life—opinions which, however prevalent they may still be among the unreflecting people of France, are daily losing ground in Germany, Scotland, and our own country, and are giving place to a more rational philosophy.

A work is in contemplation, and it will probably be shortly laid before the public, entitled "History of the Helvetian, Austrian, Apennine, Pyrenean, and Northern Floras," considered with respect to the points of origin from which the different families of plants have travelled to the valleys and plains, and become mixed together; illustrated by a Botanical Map of the regions assigned to each.

X. *Intelligence and Miscellaneous Articles.*

To Mr. Tilloch.

Sir,—I write chiefly with a view to correct the latter paragraph of the description of "Steele's Nooth's apparatus," which should stand thus: "The impregnation is very soon effected, as the pressure is great; and as the parts are fitted by accurate grinding, much trouble and inconvenience are saved, from the usual method of luting being avoided."

I think

I think it extremely hard that our feelings should be lacerated by the obstinacy of prejudice or error in neglecting to use "the Davy" in mines subject to the fire-damp.—The accident at the colliery near Durham is a sad example of the too palpable truth, that we have yet much to encounter in its universal adoption. Much pains have been taken indeed, by persons who ought to have known better, to unhinge the mind in the belief of its absolute safety. I am ready at any time to prove, at the risk of my life, that it yields a perfect security to the miner. I have lately had a most decided proof of this in one of the collieries at the Hurlet near Paisley. The mine in question had been abandoned *upwards of twelve months*, by reason of the accumulated and still accumulating fire-damp. The experiment afforded a spectacle of the most beautiful and impressive kind,—The gradual approach to the confines of the explosive waste was indicated by the included flame of the lamp presenting a lengthened spire, so as ultimately to brush the dome of the cylinder;—on passing this boundary the wire-gauze became suddenly *red hot*, and the flame of the wick was enveloped by the apparition of a foreign flame which continued to fill the cylinder—a candle here might have proved as destructive as any upon record; for in an extent of three or four acres, it exhibited from *the floor* up, an explosive medium. These mines had some years ago to record an accident by which seventeen human beings were consigned to eternity.

There was a phænomenon here which forcibly impressed me. The degrees of the fire-damp and explosive measure, as indicated by "the Davy," proved that they were not uniform in diffusion, but existing *in strata or clouds* throughout the atmosphere of the mine.

From some recent experiments, on the subject of which I may again address you, I am of opinion that the principle of safety in this wonderful instrument is to be attributed to the *depolarization* of the flame by the wire-gauze.

I am respectfully, sir,

Your most obedient servant,

Greenock, July 22, 1817. J. MURRAY.

PRESERVATION OF MEAT.

Don Eloy Valenzuela, curate of Bucaramanga in South America, has discovered that meat may be preserved fresh for many months by keeping it immersed in molasses.

STEAM ENGINES IN CORNWALL.

According to Messrs. Lean's Report for May and June, the following were the respective quantities of water lifted one foot high

high with one bushel of coals, by the engines annexed, during these months.

Work performed in May.	*Pounds of water.*	*Load per square inch in cylinder.*
25 common engines averaged	23,107,534	various.
Woolf's at Wheal Vor ..	not reported.	....
Ditto Wh. Abraham ..	52,349,333	15·1 lib.
Ditto ditto	24,713,750	3·7
Ditto Wh. Unity ..	34,928,030	13·1
Dalcouth engine	44,205,739	11·2
United mines	36,874,193	16·2
Wheal Chance	39,589,154	13·0
Work performed in June.		
23 common engines averaged	22,206,996	various.
Woolf's at Wheal Vor ..	38,438,168	15·4
Ditto Wh. Abraham ..	40,135,339	15·1
Ditto ditto	22,577,264	3·7
Ditto Wh. Unity ..	30,740,843	13·1
Dalcouth engine	41,484,504	11·2
United mines	34,298,994	17·9
Wheal Chance	32,615,890	13·0

DEATHS.

It is with regret we have to announce the premature death of our much-valued correspondent George John Singer, esq. author of "Elements of Electro-Chemistry." His death was occasioned by pulmonary consumption, and took place on the 28th of June, in his 31st year. This distinguished philosopher began to teach the sciences at an age when other men are commencing their studies. His patient and investigating spirit, combined with great mechanical skill and unwearied industry, enabled him to make some very important improvements in the instruments used in electricity, and that science owes to him some valuable discoveries. His work, which has received the honours of a double translation into French, will remain a lasting monument of his talent—it may safely be pronounced the best manual of the subject it embraces.—In private life Mr. Singer's virtues endeared him to all who had the honour of his acquaintance, among whom were some of the most able philosophers of the age. This circle might have been greatly enlarged, but that he loved retirement and privacy, more than those who delighted in his society could have wished. In him science has lost an arduous and highly-gifted votary, the community a most valuable member, his friends an inestimable treasure.

M. Werner, the celebrated mineralogist, died at Dresden on the 30th of June, at the age of 67. He has bequeathed his excellent collection

collection of minerals, consisting of more than 100,000 specimens, and valued at 150,000 crowns, to the Mineralogical Academy of Freyberg.

Dr. Spurzheim having finished his Course of Spring Lectures on the Brain, set off on Monday the 21st of July for Paris. The period of his stay in France is uncertain.

LIST OF PATENTS FOR NEW INVENTIONS.

To Thomas Wedlake, of Hornchurch, Essex, for certain improvements on ploughs.—Dated 5th July 1817.—To specify in 2 months.

To David Brewster, LL.D., of Edinburgh, for a new optical instrument called The Kaleidoscope, for exhibiting and creating beautiful forms and patterns of general use in all the ornamental arts.—10th July.—2 months.

To Captain Samuel Brown, R.N., for his improvement in the construction of a bridge, by the formation and uniting of its component parts in a manner not hitherto practised.—10th July.—6 months.

To William Henry Simpson, of Bickington, Devon, for certain improvements in the machinery for the spinning of wool, cotton, and other fibrous substances.—10th July.—2 months.

To Richard Farmer Brain, of Salford, Lancaster, brewer, for an improvement or apparatus calculated to obtain or generate gas in a more œconomical manner than heretofore, from coal or any other article, material, or substance, for lighting or heating houses, manufactories, or other places where light or heat is required.—10th July.—6 months.

To Henry Tritton, of Clapham, Surrey, for his apparatus for distilling.—15th July.—6 months.

To Thomas Aspinwall, esq. of Bishopsgate Church-yard, London, for an elliptic valve-pump box, communicated to him by a certain foreigner residing abroad.—16th July.—6 months.

Astronomical Phænomena, August 1817.

D. H. M.		D. H. M.	
2. 0. 0	♀ 215 Mayer * 12′ N.	18. 5.25	☽ α ♎
4. 0. 0	☾ apogee	19. 3. 4	☽ κ ♎
4. 0. 0	♀ 223 Mayer * 6 N.	19. 7.45	☽ λ ♎
5. 9.58	☽ ♂	20.19.31	☽ θ Ophiuchi
6. 4 39	☽ A ♉	22. 4. 6	☽ φ ♐
8 0.15	☽ 125 ♉	22. 8. 2	☽ σ ♐
8. 0.16	☽ 132 ♉	23. 2. 1	☉ enters ♍
14.15.36	☽ ν ♍	25. 3.11	☽ ε ♑
15.18.51	☽ γ ♍	30.12.19	☽ α ♓
17. 0. 0	☽ perigee		

Meteoro-

Meteorological Observations kept at Walthamstow, Essex, from June 15 *to July* 15, 1817.

[Usually between the Hours of Seven and Nine A.M. and the Thermometer (a second time) between One and Two P.M.]

Date.	Therm.	Barom.	Wind.
June			
15	57 64	30·22	NW.—Sun and *cumuli;* fine day; fine clear evening; *stratus* NW.
16	47 67	30·33	N—SE.—Very fine morning; fine, hot, sun and wind; stars and *cirrostratus.*
17	52 69	30·11	SE—E.—Clear and calm; very fine day; some *cirrus* at 6 P.M.; clear night.
18	63 83	29·77	SE—E.—Clear sunshine; *cumuli* NW horizon; fine day; moon, stars; *cumuli* E; and *cirrostratus* NW.
19	67 70	29·77	SE.—Clear and *cumuli*; fine day; very hot; clear and *cirrostratus* NW.
20	67 84	29·78	N—SE—E.—Clear and hot fine day; remarkably strong dew till late in the day in the shade; clear, calm night, and very hot.
21	67 86	29·88	N.NE—E.—Hazy and sun; fine hot day; clear; *stratus* NW; *corona* round the moon.
22	67 82	30·10	N.NW—NE.—Hot sun and windy; fine day; clear, and *cirrostratus.* Moon first quarter*.
23	63 80	30·10	N—E—N.—Sun; wind; and *cirrostratus;* great wind, and *cumuli;* fine day; clear, and *cirrostratus.*
24	62 79	29·98	N—NW.—Hazy; no sun visible; fine hot day; 8 PM. thunder and great rain; clear, and *cirrostratus.*
25	63 77	29·99	NW—W.—Cloudy; hot sunny day; clear moon- and star-light.
26	58 75	29·97	NW—SE—NW.—Very hazy; slight rain; fine hot day; clear and clouds.
27	69 76	29·65	NE—E—W.—Hot, sun and wind; sun and clouds; 6 P.M. great storm, thunder and lightning; rain and hail, and remarkable sky; black *nimbus* which hung like a curtain NW.; at 7 P.M. the sun shone between the clouds and dark *nimbus* all around; cloudy night.
28	59 70	29·60	NW—W.—Fine, and clear, and windy; fine day; showers after 3 P.M.; clear night at 10½ P.M. Full moon.

* Cats retired under trees into the *shade* to sleep (a sign of *uncommon heat*) frequently since the 19th of this month.

June			
29	60 73	29·98	S—SE.—*cumuli*, and clear; fine day; *cirrostratus* and windy.
30	63 69	29·76	S.SE—NW—SE.—Cloudy; some showers; sun and clear; fine day; clear and windy.
July			
1	60 61	29·75	SE.—Clear and *cirrostratus*; rain after 10 A.M. and windy, and very damp all day till about 5 P.M.; cloudy and great wind.
2	59 66	29·83	N.—Clouds, and stormy wind; fine day; clear and *cirrocumuli*.
3	56 70	29·87	W—SE—E.—Cloudy and hazy; fine day; showers; *cumulostratus*, and wind.
4	59 67	29·65	SE—SW—W.—Rain; clear and clouds; fine day; clear, and *cumuli*.
5	55 69	29·65	NW—W—SW.—NW. — Rain; fine day; showers; [a thunder storm at Clapton]; clear, and dark *stratus high* NW.
6	55 70	29·66	S—SW.—Rainy morn; showery; sun and clouds; clear, and *cirrostratus*. Moon last quarter.
7	55 68	29·67	W—W.—Sun and *cumuli*; fine day; clear, and *cirrus*, and stars.
8	56 69	29·88	N.—Clear, clouds, and wind; fine day; star-light.
9	59 73	29·88	W—SW.—*Cirrostratus*; clear; sun and wind; fine day; *cirrostratus*, and clear.
10	59 77	29·88	SE—S.—Sun and *stratus*; fine day; some drops of rain after 5 P.M.; star-light.
11	62 70	29·77	SE—SW.— Clear, and *cumuli*; fine day; star-light and windy.
12	59 70	29·78	NW—N.—Slight rain early; sunshine and windy; clouds and wind.
13	59 67	29·98	S—SW—W—SW.—Hazy and sun; fine day; showers between 4 and 6 P.M.; cloudy and windy.
14	55 63	29·76	S—SW—W. — Hazy and windy; slight showers and sun; cloudy and windy. New moon.
15	52 64	29·32	SE—NW—N.—Very great rain; Sun and showers; black *nimbus* and fog at 8½ A.M.; showery.

The wind is set down by a weathercock *accurately* fixed to *due* north and south, and not by one fixed by a compass, but by the meridian, by Mr. Thomas Forster, at the altitude of about one hundred feet.

METEORO-

METEOROLOGICAL JOURNAL KEPT AT BOSTON, LINCOLNSHIRE.

[The time of observation, unless otherwise stated, is at 1 P.M.]

1817.	Age of the Moon	Thermometer.	Barometer.	State of the Weather and Modification of the Clouds.
	DAYS.			
June 15	1	58°	30·26	Cloudy
16	2	64°	30·40	Very fine
17	3	68°	30·15	Ditto
18	4	71°	29.86	Ditto
19	5	82°	29·91	Ditto—thunder storm and heavy rain at 3 P.M. for half an hour
20	6	82°	29·95	Ditto
21	7	78°	30·27	Ditto
22	8	76·5	30·19	Ditto
23	9	75·5	30·14	Ditto—rain at night
24	10	72°	30·3	Cloudy
25	11	78°	30·6	Very fine—thunder storm and rain [3 P.M.
26	12	70·5	29·94	Cloudy
27	13	63°	29·75	Ditto
28	full	67·5	29·80	Ditto
29	15	68°	30°	Very fine
30	16	58°	29·77	Rain—heavy thunder storm and violent shower of hail and ice
July 1	17	64·5	29·77	Rain
2	18	59·5	29·70	Cloudy—blows hard from S.W.
3	19	69°	29·98	Ditto—rain at night
4	20	71°	29·71	Ditto ditto
5	21	57°	29·70	Ditto ditto
6	22	65°	29·76	Showery—ditto
7	23	68°	29·30	Cloudy
8	24	62°	29·93	Ditto
9	25	64°	29·99	Very fine
10	26	72°	29·94	Cloudy—rain at night
11	27	66°	29·89	Rain
12	28	62°	30·5	Cloudy—rain at night
13	29	63°	30°	Showery—heavy ditto
14	new	68°	29·70	Ditto

There has been a great deal of rain fallen since the 1st July; and thunder almost every day in the past month. It is to be observed that the thermometer is hung against a wall upon which the sun never shines. The 20th June it rose to 91° in the sun removed from any thing which could reflect and increase the heat of the air.

METEOROLOGICAL TABLE,

By Mr. Cary, of the Strand,

For July 1817.

Days of Month.	Thermometer. 8 o'Clock, Morning.	Thermometer. Noon.	Thermometer. 11 o'Clock, Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
June 27	68	74	64	29.55	52	Showery
28	63	72	58	·72	71	Fair
29	60	70	62	·80	57	Fair
30	64	68	55	·69	52	Showery
July 1	60	60	53	·50	0	Rain
2	52	66	57	·65	45	Fair
3	58	66	57	·80	36	Showery
4	57	64	54	·60	38	Cloudy
5	59	66	57	·50	31	Cloudy
6	57	67	56	·62	46	Fair
7	61	67	55	·73	55	Fair
8	61	70	61	·80	70	Fair
9	62	70	60	·80	34	Fair
10	62	74	61	·78	67	Fair
11	64	70	62	·75	45	Fair
12	62	76	63	·90	37	Showery
13	62	67	57	·80	32	Showery
14	60	64	56	·58	32	Showery
15	62	64	54	·20	30	Showery
16	55	62	55	·62	52	Cloudy
17	56	67	56	·79	55	Fair
18	55	63	54	·78	41	Fair
19	56	66	54	·88	47	Cloudy
20	60	67	55	·88	42	Cloudy
21	59	69	60	·86	46	Fair
22	60	67	60	·80	38	Showery
23	60	68	60	·84	36	Cloudy
24	60	70	60	·99	61	Fair
25	62	70	58	·92	62	Showery
26	61	65	57	·69	0	Rain

N.B. The Barometer's height is taken at one o'clock.

XI. *On the Cause of Ebbing and Flowing Springs.* By Gavin Inglis, *Esq.*

To Mr. Tilloch.

Sir, — As you have again brought into notice the ebbing and flowing spring of pure fresh water in Bridlington harbour in the 227th Number of your valuable Magazine, I beg leave to send the substance of some observations intended to have been submitted to you at the time Dr. Storer's communication to Sir J. Banks was published in your xlvth volume, page 432.

Dr. S., after relating the circumstances which led to the discovery of the spring, says: "As soon as the surface water in the harbour during the flowing of the tide has arrived at a level of 49 to 50 inches lower than the top of the bore, the water begins to flow from it in a stream equal to its calibre; the impetus of which is increased as the tide advances, and may be observed to be propelled with much force after the bore is overflowed by the tide. The discharge continues from four to five hours, *i.e.* till the tide in returning falls to the same level at which it began to flow.—The rule appears to be, that the column of spring water in the bore is always supported at a height of 49 to 50 inches above the level of the tide at any given time."—"Such is the state of facts," continues the Doctor; "and it appears to open a subject of curious investigation to those whose habits and practical knowledge qualify them for it. The appearances seem, not to admit of any satisfactory explanation, without supposing some mode of subterranean communication, by which the water of the sea and that of the spring in question are brought into actual contact so as to exert a reciprocal action."

I beg leave to differ from the Doctor in supposing the rise of the fresh water above the level of the tide to proceed from these waters coming into actual contact, upon the principle of two liquids of different specific gravities in an inverted syphon.

The facts themselves are at variance with this hypothesis.

The well-known specific gravity of the German Ocean does not so far exceed that of pure spring water as to equal a column of 49 to 50 inches of superior altitude. The stratum of very solid clay, the tapping of which procured for Bridlington this wonderful supply of very fine water, will be found upon examination to extend not only from Flamborough Head by the Smithwick Sands to Spurn Point, but to underlie Bridlington, the whole Wolds behind, and up the country till it runs out and is succeeded by that cretaceous gravelly soil whose dipping stratum occupies the intermediate space betwixt the clay and the rock. To this alone is to be attributed the want of water in the Wolds; the

few streams or rivulets; their lowness in summer and dryness in autumn; their regular supply being only what oozes or percolates through the clay. The substratum of gravel will be found to be the common receptacle of all the waters that fall in the upper country, and which would otherwise flow in copious springs and streams over the wolds, &c.

The Gipsies will be found mere perforations of the superstratum of clay; and one and all of them at some seasons, although distant from the sea, to be less or more ebbing and flowing springs. These begin to flow copiously, after the frost has so far penetrated the upper mould or turf as to solidify the surface of the clay, and prevent all further oozings of the water from below; then the accumulation of waters in the substratum must increase with great rapidity, become irresistible, and propel themselves with force from every opening; which projection will increase at all times with the flowing tide, and be at the highest at full sea, lessen in proportion as the waters of the ocean recede, leaving the flexible clay to give way to the hydraulic pressure from below when freed from the weight of waters above. Clay, however solid (in an unburnt state), when moist is an elastic substance; and, in fact, that whole bed extending from Flamborough Head to Spurn Point will be found to rise and fall with the ebbing and flowing of *every* tide. When the recess of the ocean, as I have said above, lessens the pressure upon the upper surface of this immense bed of clay, whose extent must in an eminent degree contribute to its elasticity, the hydraulic pressure on the under stratum, by waters from an unknown altitude, must raise the whole mass in proportion as the force is superior to the resistance. The return of the tide brings with it the weight and altitude of its mass of waters, and unavoidably acts on the flexibility of the clay, as a pressure would on an hydraulic blowpipe; and of course "sets up the Gipsies," whose rise, in a calm, will be progressive and smooth. But in a storm, the clay, shaken by the thundering violence and beating of the waves, must occasion the consequent undulation of the water from the springs, by its elastic vibrations. When the collection of waters from above is greater than the natural discharge of these gatherings, by the fissures in the rock at the back of Smithwick Sands, then the Gipsies must get up, and the springs will naturally flow higher and longer every tide, than when the collection is little more than the natural discharge.

To Bridlington this discovery has been of great advantage. But there is a result of infinitely greater consequence to that town and neighbourhood than the mere production of pure fresh water for the ordinary purposes of life. By sufficient tapping, the Wolds might be rendered inestimably valuable and productive,

tive, comparatively speaking, by giving free vent to the waters from below the clay, instead of leaving it to ooze through, which keeps the soil always *weeping*;—consequently damp, cold, and unproductive. In an age of improvement like the present, it is to be wondered that this has not been attended to. There is no mode of draining a clay soil equal to boring, particularly when lying on a substratum of gravel: whenever this is the case, water may always be procured by boring in the dipping of the gravel stratum; on the contrary, by boring in the cross levels, a stream may be turned into the bore, and disappear. Hence the Scotch phrase of "driving the bottom *out* of a well," by sinking too deep.

St. Winifred's or *Holy Well* in Flintshire is the discharge of waters collected under similar circumstances; and probably at no great distance from its source, the waters being muddy and whey-coloured after heavy rains. These waters now, instead of working miracles, are turned to a more rational though perhaps not a more profitable account,—that of turning useful machinery: and I have no doubt whatever, but by sinking or boring, and casing with cast-iron boxes, a quantity of water might be procured, in the neighbourhood of Bridlington, sufficient for, and which might be most profitably applied to, the working of *even heavy* machinery, either by applying the water direct from the pit or bore, raised sufficiently to cover the wheels of machinery, or by throwing it into reservoirs, and applying it in proportion to the weight required for the machinery to be driven.

It is impossible to conceive to what extent this might be carried, and to what a pitch of commercial greatness this simple discovery may raise Bridlington.

Strathendry Bleachfield, Fife, July 22, 1817.

XII. *Report of the Select Committee appointed to consider of the Means of preventing the Mischief of Explosion from happening on board Steam-Boats, to the Danger or Destruction of His Majesty's Subjects on board such Boats.*

[Continued from p. 65.]

The Evidence of Seth Hunt, *Esq.*

Are you concerned with the province of Louisiana?—I have been in Louisiana; I formerly was commandant in Upper Louisiana.

Can you furnish the Committee with any information in respect of the safety of steam-boats?—In the United States a great

number of steam-boats have been established: The first was at New-York; there are now running between New-York and Albany, ten boats; two between New-York and the State of Connecticut; four or five to New-Jersey; besides the ferry-boats that pass and repass across the river, of which there are four; those boats work all by low pressure engines; no accident has ever happened to any one of them; they have been running since the year 1807; and the boats at Albany perform about forty trips each per annum.

What distance is that?—An hundred and sixty miles. They go up in twenty-one hours, and come down in nineteen; sometimes a little longer, but never shorter than nineteen; that is the quickest passage.

At what rate per hour do they go?—Some of them go about seven miles an hour in still water; some boats have gone nine, ten, or eleven knots; but that is under particular circumstances. They have come from Newhaven to New-York, ninety miles, in six hours and a half, without any sail.

Do they ever make use of a sail?—They have a sail and a mast, which they can lower down and raise up to take advantage of a favourable wind, to assist them in their passage.

Those boats are upon rivers?—Those which go to Albany pass up the North River, and the others to Connecticut pass through what is called Long Island Sound, which is forty miles broad in one part of it. On the river Delaware there are a number of boats also established, which ply between Philadelphia and Trenton, in New-Jersey; and Philadelphia and Bordenton, in New-Jersey; also others between Philadelphia and Newcastle, and Philadelphia and Wilmington; beside ferry-boats which pass and repass the Delaware. Several of those boats have low pressure engines, others have high pressure engines, working the high pressure engines from 100 to 140 pounds the square inch, and as high as 160; but those engines are constructed upon Oliver Evans's plan, called the Columbian plan.

Are they of wrought iron?—Yes; there are no cast-iron boilers in America. I presume that may arise from their not having foundries in which they can cast them sufficiently large; they are all wrought-iron boilers or copper; all which have to pass through salt-water are copper. The boat Etna, which passes between Philadelphia and Wilmington, is a high pressure engine, and outstrips all the other boats; there is no competition at all between them. There are boats which pass also on the Chesapeak, which is there forty miles wide; they pass from Baltimore to French Town and back, regular boats, two lines of boats; one leaves Baltimore one day and the other the next; they pass

every

every other day alternately. There are other boats from Baltimore, which go to Norfolk; there they pass a still wider part of the Chesapeak, which may be sixty miles wide; they have been to New-London, which is still more exposed; and have been up to New-Hertford.

Are those with high pressure engines?—No; low pressure. On the Potowmac there are also steam-boats, and on the James River, which pass between Richmond and Norfolk.

Have any accidents been known to arise on account of the heavy seas?—No; no accident whatever. I have not mentioned the most important circumstance connected with this:—the Powhattan steam-boat was built at New-York, went into the open ocean, encountered for three days a very severe gale of wind, arrived safe at Norfolk and up to Richmond. The gentleman is now in England who navigated her; and I have heard him say, that he felt himself as safe as he should in a frigate; and he said there was this advantage, that the steam power enabled him when they could not have borne sails, to put the head of the vessel to the sea, instead of lying in the trough of the sea, being exposed to be over-run by the waves.

What was her tonnage?—Two hundred and fifty tons.

What is the largest steam-boat in America?—The largest I have seen are those on the Mississipi, the Etna and the Vesuvius, which ply between New-Orleans and the Naches; they are 450 tons, and they carry 280 tons merchandize and 100 passengers; 700 bales of cotton besides the passengers are transported to New-Orleans.

Have you any regular allowance of power according to a ton? —I believe that after they have proved their boilers, which I presume should be done in all cases, if they wish to ascertain the pressure, they work with safety at half that which it has been proved at.

Is there any rule according to tonnage established as an usage? —I am not an engineer, and am not conversant with that subject; I have passed through the country, and have been on board most of those boats, but I am not acquainted with that fact.

Have any accidents happened?—Within my recollection only three accidents have happened to steam-boats in America: the first happened on the Ohio, and was occasioned, as stated by the public papers, by the negligence and inattention of the engineer, who loaded the safety-valve, and neglected to attend the fire; all hands were engaged in hoisting the anchor, the fire was in a very high state, and of course produced a vast deal of steam that did not escape by the ordinary operation of the engine, which would discharge it and carry it off.

What is called the safety-valve had been improperly loaded and neglected?—Yes, but that never need happen; the principle of steel-yards is to put a weight at the end, and if you put no more than that, it will answer its purpose;—so with a steam-engine; it may be overloaded, and its effect destroyed. The next accident happened, not from a fault of any body, but from an act of God; it was lightning, as was satisfactorily explained to the public, both by the passengers and those interested in the boat; that was at Charleston in South Carolina; the pipe which carries the smoke up to the top attracted the lightning, and it went down and split the boiler.

It was not considered as at all connected with the operation of the engine?—No, not at all through negligence. A third accident happened lately to the Powhattan; she was not in operation when it happened; they were out of fuel, they stopped their boat and lay still upon the water while they went after wood; still however they kept up their fire, and the steam was high, and it exploded in that situation, there being no consumption of the steam as it accumulated. Those are the only accidents that ever happened, except such as have happened from vessels taking fire.

Were those vessels high or low pressure engines?—All low pressure engines. No accident has ever happened in America to a high pressure engine, either in a manufactory or out of it; and there are many engines used in the manufactories, and in flour-mills and saw-mills, constructed upon the plan of Oliver Evans, which act on the high pressure principle to 150 pounds an inch; he has worked 160, but 120 is his constant average. There is not an old woman in America that is ever frightened at all at a high pressure engine, any more than they now are at a cannon. There is a very large engine, about a forty-five horse power, at St. Sennati, on the Ohio River, which moves seven pair of stones in a flour-mill, a woollen manufactory, and a cotton manufactory seven stories high; it works upon the high pressure, and there are saw-mills and grist-mills at various places.

What is the fuel?—Wood in most places. At Pittsburgh and on the Ohio River it is coal and wood; at Pittsburgh and at Weeling, and a hundred other places, there is fifty miles square a solid mass of coal; they drive the shaft horizontally into the hill, and the coal is abundant above their head in the mountains, as fine coal as any in the world; it is delivered at the houses of the inhabitants at sixteen bushels for a dollar.

Is the number of steam-boats now increasing in America?—Very rapidly.

Are those that are now constructing upon the high or the low pressure system?—Upon both, because there are different interests

terests and different companies. Mr. Evans being a patentee, they have to give something for the use of his patent;—if they cannot make their bargain with him they use the low pressure engine; but there is a new engine invented in America, a perfectly rotatory engine, built for one-third of the money, which is now coming into use in several of the steam-boats; and it was supposed when I came away it would supersede all other engines.

Do you know of any particular guard in the construction of steam-engines used in America to prevent accidents?—I know of no other than that of properly constructing the safety-valve, and the manner of loading it, so that they cannot get on more than a certain weight; they must of course construct them strong enough and prove them.

They are under no Government regulation?—They are not.

Does that with a rotatory motion consume more coals?—It is supposed to consume less; twelve bushels of coals with the rotatory motion will perform the same work as the other engine with twenty.

Mr. TIMOTHY BRAMAH'S *Evidence.*

You are an engineer, at Pimlico?—I am.

You were one of the gentlemen that went to Norwich to inquire into the explosion of the steam-boat?—I was.

Did you go at the request of any party, or of your own voluntary suggestion?—I went in consequence of my friends, Mr. Collinge and Mr. Donkin, calling upon me to ask my opinion, whether it would be right for us to interfere upon such an occasion; I concurred with them that it would, and volunteered to go.

Your design was to inquire into the causes of the explosion?—Yes, and to examine as much of the wreck as we could find.

State to the Committee to what you attribute the accident?—The observations I made led me to determine it was owing to the expansive force of the steam, and the inadequacy of the boiler to sustain that force.

Was it a high pressure or a low pressure engine?—A high pressure engine; the boiler was badly constructed and shaped.

Of what materials was the boiler composed?—Of wrought and cast iron, and it was the cast-iron part that gave way.

Those two materials expand in a different proportion with the same degree of heat?—Yes, they do.

Is it usual to have the boiler of wrought and cast iron?—I should think it would be avoided on all occasions by experienced engineers; but I have often seen it.

This engine was not made so at first, but altered afterwards; was it not?—Yes, in consequence of the other giving way.

Have you any reason to suppose that the accident might be attributed to negligence or mismanagement in the director of it?—We had verbal testimony, from which I had no doubt the steam was at a considerable degree of pressure; but the end was very improperly made.

Did you ever learn at what rate the man was working?—No, I understood he was working at sixty pounds an inch generally, probably it might be 120 at that moment; but I should think it not equal to the working of sixty, for it was only three-fourths of an inch, and a sixteenth in some places in thickness, and it was four feet in diameter at the end; it was a flat end to the cylinder like a drum.

Is it possible to construct the engines in steam-boats in such a manner that there is great improbability of any accident happening?—I do not know how to answer as to their being perfectly safe; I do not feel that materials, when they are submitted to so great a pressure, are safe, for we find that very few materials will stand a great degree of pressure for any length of time; we often find that a water-press, which has been efficient six or seven years together, at length gives way, when the metals are subjected to a very great pressure; it is like a blow with a heavy machine for breaking metals, which does not break the first time, but is constantly tending to loosen the particles.

Do you think that a high pressure engine, under any guard that can be applied to it, is a safe engine to use in a steam-boat?—I do not conceive it is a proper engine, or a safe one.

Did you ever hear of their having been used with wrought-iron boilers with perfect safety?—No, I have not heard of any comparative statement of either the wrought or cast; I know they are usually made with cast.

Do you consider yourself sufficiently an engineer, with respect to the construction of steam-engines, to be able to give of your own knowledge, a decided answer to such questions?—Yes, I do conceive so; I have paid a great deal of attention to the subject of steam-engines, and I believe I know the principle of every one in existence.

If on a certain pressure in a high pressure engine, a safety-valve or safety-valves were so constructed, as that they would open and discharge the steam with a pressure much less than the boiler was calculated for, would not such a boiler be perfectly safe, admitting it to be made of proper materials and properly constructed?—Yes, if it could be proved that the boiler was calculated to resist a pressure much greater than that to which

which it was to be subjected in the ordinary way of business, and that proper safety-valves were applied, it would be safe as long as the action of those safety-valves were insured, and so long as the perfection of the metal could be upheld.

If a boiler was found to sustain the pressure of 100 pounds to a square inch, and such boiler had been tried, and it was found, before used, that it would bear a pressure of 200 pounds upon the inch, would not such a boiler be perfectly safe to be used, if the safety-valve was so constructed as to open itself at the pressure of sixty?—I cannot pronounce it perfectly safe, and I must give this reason;—I think if a boiler was prepared to sustain 100 pounds, and strained to 200, it might afterwards perhaps burst at forty, the straining having injured it.

In the situation of steam-boats, might not the unskilfulness of the sort of persons who manage them render any steam-boat unsafe?—I do not know how that could be the case; they might by wilful perversion of the proper principle of management render them unsafe to a comparative extent; for instance, if there was half the pressure there would be but half the danger under like circumstances.

Do you or not apprehend, that a boiler upon a proper construction, of wrought metal, may be tried with a certain force, so small in comparison with that pressure which it is intended to bear, as not to incur any risk of being injured in the proof, and have a complete surplus of strength, so as to enable it to be afterwards used without any danger in the use?—I should pronounce such a boiler to be perfectly safe, and so long as it maintained those properties it would continue so.

Have you considered how safety valves may be constructed as adapted to boilers, so as to put it out of the power of the person having the management of them improperly to load them, or to alter their nature?—The most simple mode which has suggested itself to me is, to have a double safety-valve, and to lock one up and to have it examined once a week, or as often as may be necessary, to see that its action is perfect.

If there were those two safety-valves, one under the management of the person who had the direction of the boat, and the other safety-valve under such guard that he could not prevent its action; such a boiler would, in your opinion, be safe?—That would be more safe than any I have ever seen.

Have you ever witnessed the different effects of the explosion in cast and wrought iron boilers?—No; I have seen wrought iron vessels that have been burst—torn out, as it were.

Did you never see a cast-iron vessel burst?—Yes, many; the wrought iron generally tears and opens out, to admit of the fluid escaping; it is generally the fluid which does the mischief when the

the wrought iron is used, and it is both the fluid and the material which does the mischief when the cast iron bursts; the effect in cast metal is, to carry the pieces of the metal to a considerable distance, which is seldom the case in the wrought, unless where there is any cold shut in the metal; the cast bursts like a shell, projecting the particles of the metal to a considerable distance.

If an accident of that nature happens to a wrought-iron boiler, the mischief would probably be confined to the room in which the boiler was placed?—No, I do not conceive that to be the case; I have no doubt, if it had been a wrought-iron boiler in this case, the deck of the vessel would have been blown off; the pressure would have been in all places alike; but here it was only in a lateral direction, and the end of the boiler was blown into the river, and by its re-action the boiler itself was thrown into the river on the other side.

You have said, that you have frequently seen wrought-iron vessels burst?—Not frequently in our own experience; I have seen copper frequently that has burst.

Have any fatal or serious accidents happened on those occasions?—I have heard of some, but have not witnessed one; the accidents I have observed have chiefly arisen where cast-iron boilers have been used.

In the first instance, when wrought-iron boilers are used, the injury is sustained by individuals by the fluid escaping?—Yes.

Where cast-iron boilers have been used, it has been by the explosion of metal?—Yes; I do not mean to say it may not be by the explosion of wrought-iron boilers; it is very difficult to obtain a boiler of perfect metal; and if there are any cold shuts, or other defects in it, it may explode in the same way.

Is copper subject to the same evils?—No; I think it is generally in a purer state; iron is very impure at the best.

Mr. John Taylor's *Evidence.*

What is your profession?—My principal pursuit is that of a manufacturing chemist, at Stratford in Essex; but I have the control of a district of copper mines near Tavistock.

Have those engagements made you perfectly conversant with the nature and application of steam-engines?—I have attended to that subject to a certain extent; of late my attention has been called to high pressure steam particularly, being concerned with my brother in a patent for applying high pressure steam to the boiling of liquids, and using it extensively in our own manufactory, both in steam-engines and for the purpose of boiling.

Are you acquainted with the accident which lately happened to the steam-boat at Norwich?—By report only.

What do you know of that transaction?—I have heard that the

the plate of cast iron was of inadequate thickness for the strain to be put upon it. With respect to the impropriety of cast iron compared with wrought, we ourselves constructed one of the first high pressure boilers we used, precisely in the same manner with that on board the Norwich boat; the boiler was proved to 100 pounds a square inch, by the water proof, commonly used with about forty pounds pressure, but the cast-iron end broke one day with less than twenty pounds pressure of steam; the fracture being caused evidently by the heat expanding the cast-iron end unequally, and being kept from going to the form it would otherwise assume.

Then you are of opinion it would be improper to make one of such a construction?—As far as I at present know, I should say it was. Upon that we altered our boilers, all having been since made of wrought iron only. I have seen most of the high pressure boilers which have been made, except Woolf's. I have seen Trevethick's old construction, which were cast iron; his new construction with his wrought-iron tubes. The Wells-street boiler, which blew up, I saw immediately after its destruction; I was surprised to see that it had been made of cast iron, a pan of eight feet diameter therefore extending the bursting surface in the proportion of four to sixteen; it was of unequal thickness, badly cast, cast from small furnaces, and the contact of the iron not complete; it did not meet in fusion.

Was that a high pressure boiler?—Yes, intended to boil sugar; the thickness was intended to be, doubtless, about two inches or two inches and a quarter, but by inserting the core unequally, the thickness on one side was three quarters of an inch, on the other side the thickness of the metal was two inches and a quarter, or thereabouts; therefore to the general objections to cast iron was added a most improper construction. I understand from the men who were working there (the Frenchmen) that there had been something like a mercurial gauge attached to it, but that the mercury never fluctuated; it indicated nothing that the safety-valve was loaded down with weights, which we could not collect, and therefore did not ascertain the pressure; but that it was probable there was a pressure of more than 100 pounds per inch.

Had you ever seen it worked before?—No, nobody was admitted to see it worked.

How many accidents have occurred in the high pressure boiler to your knowledge?—The first I ever heard of was one of Trevethick's at Woolwich.

Was that a cast-iron boiler?—It was. In that case the safety-valve was a very awkward thing, hardly to be called a safety-valve; he himself was not awakened to the danger till that accident

cident happened. The second case that I heard of was in the North, a propelling engine (it was mentioned in all the papers); it was near Sunderland, of a boiler driving waggons; the facts of the case I know to be these, from the engineer who made the boiler.—In the first place, they had a smaller boiler to the same engine; that boiler did not generate steam so fast as the engine could expand it, consequently there was never an excess of steam came out of the safety-valve, the engine-man therefore with impunity screwed down his safety-valve; it was never used. The proprietor of the engine wishing to have more power, ordered a larger boiler, which had the power of generating nearly double the quantity of steam; this was sent, and a caution given by the gentleman not to attach it to the engine till he arrived; but that was not attended to; the boiler was attached to the engine; the man went to work as before, and he screwed down his safety-valve, not knowing, that though before he had a deficiency, he had now an overplus; he said he would make a good start of it; the boiler exploded, killed several people, and him among the rest; and the force was remarkable, as shown by the fragments of coal that were driven through the men's clothes or into their bodies from the tram. The Wells-street was the third case; the safety-valve was loaded in this case. At Norwich I apprehend the safety-valve was loaded. The only other case was in Trevethick's new high pressure boiler, the wrought-iron boiler; that, I should say, was something like a boiler formed of two arcs of circles; it burst without doing any hurt, and perhaps the circumstance is not known to ten people besides myself. The people were near it, and it did them no hurt. The reason it burst was, that a man very ignorantly took out bars which he should not and altered its construction. These are the only instances I know of the high pressure boilers.

Do you consider low pressure boilers as safe from explosion under all circumstances?—Only owing to the column of water that fills them; that is the only reason I consider them as safe.

If they are supplied by a column of water, then do you consider them as safe from explosion?—I do not consider them as absolutely safe, because I know facts of their bursting; in case of their not being fed with a column of water they are very unsafe; for the construction of the boiler is weak in itself, and you have no dependence but upon a safety-valve, which may be loaded improperly.

Do you conceive that a wrought-iron boiler may be rendered safe under all circumstances?—I do consider that it may.

State how?—Principally by the use of a column of mercury in a syphon or tube, of sufficient size; when that mercury is displaced by the expansive force of the steam, which would be regulated

gulated by the height of that tube to admit of the efflux of the steam from the boiler as fast as it was generated by the fire, in that case the expansive force could not increase in the boiler, but the mercury would be blown out and the steam would escape: that I consider one of the best securities to the boiler. Besides the common safety-valve, which may be at the discretion of the workman, I conceive it essential to have another safety-valve, which is under the control of the master or proprietor of the works. There is another small contrivance, which I consider very important to the safety of the boiler. Boilers have been weakened very much by the water having been evaporated too low, so that the bottom begins to be acted upon by the fire and weakened. A hole having been previously bored in the bottom where the fire acts, may be riveted by a piece of lead, so that that lead remains perfectly secure as long as it is covered with water, but the moment the water leaves it the lead melts; the steam is blown out through the hole and puts out the fire; besides giving the signal of what is wanted, it at once puts an end to the cause of danger.

Do you consider that the mercurial gauge acts in any other manner than as a safety-valve, which cannot be stopped or put out of order?—It does not act only in that manner, but it has the advantage of exhibiting during all times of the boiler's working, the state of the steam within the boiler, by the fluctuation that takes place in that column, as indicated by the index upon the surface of the mercury, and the state of that mercurial gauge is observable every moment. If the mercury becomes stationary, one would strongly suspect that that tube was stopped, therefore it would point out itself instantly that it had become not what it ought to be; the safety-valve has not that advantage, as it does not indicate any thing till the steam is blown out by raising the weight.

An observation of the mercurial gauge by an intelligent person, would tend to guard against mischief?—Yes, by *any* person.

What are the different effects produced by the explosion of cast and wrought iron?—As far as I have stated the fact with respect to Trevethick's boiler, which was of wrought iron, a rent or fissure was produced, and the form of the boiler was disfigured, but no fragments were thrown about so as to produce any serious injury.

Do you conceive that to be the usual effect?—I conceive it would be the effect; and I conceive further, that one might predict with some degree of certainty where that fissure would take place; it would take place in that part of the boiler that is most exposed to the action of the fire, that growing thinnest.

Have

Have you ever seen an explosion of a cast-iron boiler?—No, I have not; I have seen the effects at Wells-street, I was upon the ruins immediately after; the effect seemed to be tremendous; there it knocked down the whole building, which was a sugar-house of five or six stories high, and fragments appeared to be thrown in every direction; the boiler itself was shattered into a great number of pieces.

If that had been a wrought-iron boiler and had burst, it would not have produced the same effect?—I think not.

Are you at all aware whether there is any preference of copper above iron, in the construction of boilers for high pressure steam-engines?—I should think that copper is the best metal of all; the most ductile.—But I think at the same time, that with good wrought iron, boilers may be made perfectly safe up to the estimated strength of from four to five hundred pounds pressure per inch.

Have you formed any opinion respecting the pressure per inch, necessary to drive a steam-boat through the water at the highest rate at which you have heard of any hitherto having gone?—I have not turned my attention particularly to the use of high pressure steam, as applicable to steam-boats. But being the owner of a high pressure engine, I see no advantage at present in going above forty or fifty pounds an inch in steam-engines.

Supposing then that a boiler were constructed, with the intention of its resisting a pressure of steam equal to 300 pounds per inch, that it should be afterwards proved with a force equal to two hundred, and that it should be after that worked with a pressure under a hundred, do you conceive that any supposable danger could exist under such circumstances?—None at all; provided the steam was limited to a hundred.

It is understood of course, that the common precautions of safety-valves, the operation of which could not be impeded, should be applied to such boilers?—Yes; with respect to the valve of high pressure steam for working engines, I beg leave to say generally, that in Cornwall of late a most valuable improvement has taken place; and that if it is an object to save coal to steam-vessels upon a large scale, I do conceive that high pressure steam becomes an object of great importance to them. I mean if applied upon the principle that Mr. Woolf has in the first place introduced, but which has been applied by Mr. Sims, and I believe by some others.

You are of opinion these high pressure boilers might be made with equal safety as low pressure boilers?—Quite so.

Do you know any thing of the saving of coal produced by high pressure engines?—I have in my hand a statement of the work done by the engines on the principal mines in the county of Corn-

wall.

wall. It states the consumption of coal, and the work done by every engine therein named, from which it appears that the average work of engines now in the county of Cornwall, is to raise about twenty million pounds of water one foot high, by the consumption of one bushel of coals; that by the introduction of high pressure steam under the best mode of management, an effect equal to from forty-three to forty-five million pounds of water is raised the same height by the same quantity of coal, thereby producing above double the effect.

Do you apprehend that condensing or low pressure engines are liable to be blown up by the carelessness and inattention of the engineer conducting them?—I apprehend equally so with high pressure engines; and I am of that opinion from facts which have reached me. In France, at Crusoe, some very good engines were erected by Mr. Wilkinson, at a very large work. They were on Bolton and Watt's principle; one of them blew up and killed several people. I have heard of other instances, but they are not within my own knowledge.

Do you conceive that the mercurial gauge may be applied with ease to the high pressure boilers, so as to produce safety, as certainly as the column of water, which is in fact a water-gauge, which is usually applied to the low pressure?—I do most undoubtedly think that, provided the mercurial gauge be of a sufficient bore; and I think, in some respects, it would have the advantage of the water-gauge, as being less liable to accidental obstruction.

Do you conceive that there is any difficulty whatever in constructing a safety-valve, so as to operate with certainty, and to be safe from any impediment which the engineer might intentionally place in the way of its operation?—I do think such a safety-valve can be constructed.

Do you apprehend any additional considerable expense would be thereby incurred?—Not any considerable expense; we have done it to all the boilers we have lately superintended the erection of, putting them under lock.

Mr. John Collinge's *Evidence.*

What profession are you of?—An engineer and iron-founder.

In the course of your profession, are you conversant with the nature of steam-engines?—I have made several.

Where do you live?—In Bridge Road, Lambeth.

I believe you are the patentee of the patent axle-tree?—I am.

Did you go to Norwich in consequence of the accident that happened to the steam-boat there?—I did, in company with Mr. Donkin and Mr. Brown.

Did you go at the request of any person?—No, it was voluntary,

tary, from an impression the public mind would be alarmed, and wish to know the cause of the accident.

Did you see the boiler, or any of the remaining part of that engine?—I did.

Do you attribute the cause of that explosion to the construction of the boiler?—I do.

Be so good as to state what it was?—The boiler was composed entirely of wrought iron, except one end, and that was capped with cast-iron.

The cylindrical part was made of wrought iron?—Yes.

It was a high pressure boiler?—It was.

Originally it had all been wrought iron?—It had, I believe.

But upon an alteration they put one end of cast iron?—Yes.

Was not such a conjunction of metals in such a place very dangerous?—Certainly.

Principally because the expansion of the metal is totally different in one and the other?—Yes.

What is your opinion, as an engineer, in respect to the material of which boilers in general should be made?—Any material under very severe pressure is liable to fail, and cast iron for this reason, because in all large bodies we find that the air cannot wholly escape in the act of fusion. I have occasionally had large masses of cylinders and pans to break up, and we find frequently cells where the air could not escape, so that we are never certain as to the solidity of cast iron; there is certainly a much greater dependence upon wrought iron or upon wrought metal; perhaps it would be better to include copper.

In wrought iron there is danger from cold shut?—Yes.

Supposing an accident should happen to any boiler, which would be most likely to be attended with the greatest mischief, a cast-iron or a wrought-iron boiler?—Cast iron, because cast iron flies off in fragments, and wrought, from tenacity, only rends.

Did you ever hear of an accident in a wrought-iron boiler when it has exploded, that has done any considerable mischief?—I was almost upon the point of believing, that wrought-iron boilers would have resisted a degree of pressure, if properly made, beyond what I find they will do; because an accident has occurred at Malden, where a boiler, nineteen feet long, was blown off from the seat of its connexion with the base. I have found, in making wrought-iron boilers myself, that if I make them of metal of a considerable substance, that they cannot be so well united to make them steam tight; it is a very difficult thing to do; how far that is the case with copper, I have no acquaintance, but perhaps it would not be precisely the case with copper; the rivets that are applied to wrought-iron boilers are put in hot, and

and when they are hammered to secure the joint, they get cold, of course they shrink, and do not fill the hole through which they have passed.

The wrought-iron boiler which you stated burst was not applied to a boat?—No, for a salt-work.

Is it your opinion, as an engineer, that any boiler, whether of wrought or of cast iron, but particularly of wrought iron, could be made, by the construction of safety-valves, so secure that all danger from it would be almost impossible?—At present I have no conception that any safety-valves could be applied to render them perfectly secure under heavy pressure.

Is it your opinion, that if a boiler was originally constructed of wrought iron, to bear a pressure of 100 pounds to the square inch, and that such boiler had been tried by experiments, say at sixty, and that a safety-valve was applied to it which should open at a pressure of thirty, such a boiler would be liable to be exploded?—Not unless it had been previously strained by the experiment to render it too weak.

Cannot a safety-valve be so made that it shall open, and be certain to open, at a particular pressure?—The safety-valves ought always to open at that pressure; but from causes that we cannot ascertain, that does not happen in cases where accidents occur; it is to be hoped that safety-valves will be contrived to answer for high pressure engines.

Would it not be possible to apply to such an engine as that a tube with a column of mercury?—Yes, and it would be a judicious application; but it requires such an altitude, I apprehend it is not very easily applicable to boats from the agitation of the vessel; but if it could be applied, it is the best application that can be made.

In the low pressure engines the general safety is by a column of water?—Yes.

That could not be used on board a boat?—No.

Then you think the mercurial gauge would be the greatest safety for a boat, if it could be applied?—Certainly; if it could be judiciously applied, it possesses greater safety than any other.

Have you seen steam-boats on the Clyde or Humber?—No.

You know those on the Thames?—Yes.

What is the greatest power that would be required?—The condensing-engines should not be more than four pounds to an inch; and if the capacity of the vessel allows of it, the condensing-engines answer every purpose, because you can have one on board more than sufficient for the tonnage; because the making a wrought-iron boiler would be on such a scale of thickness, that if more than the usual pressure was applied, the rivets would fail, and constitute a security against any fatal occurrence.

 Could

Could not a boiler then be made for what they call a high pressure engine, equally safe?—I should apprehend not, for the reasons I have stated: I have made several boilers, and I find if the plates are thick beyond the dimension usually employed for condensing-engines, that they do not prove equally steam-tight.

Explain whether you mean the plates or the seams?—I mean that the seams are not equally steam-tight.

Did you from any report you heard, besides the bad construction of the boiler at Norwich, discover that any negligence was imputable to the direction of that engine?—It was presumed by report that he was imprudent frequently; for the purpose of impelling his vessel with greater force, that he did load his engine too much.

Did you see any body who had escaped from that accident who was on board the boat?—I did not.

If there was too much weight added to the valve, would not that occasion the explosion?—There is no doubt that was the case; but a much smaller degree of pressure would have burst a boiler so constructed.

Then if a boiler had been made properly, and a man had been so imprudent as to have loaded the safety-valve, the same accident might have occurred?—Certainly.

You have said, from the power that was wanted with regard to steam-boats, you thought condensing-engines were the best engines applicable for that purpose?—I think so, no doubt.

Do you mean the best as applied only to safety, or for use?—For safety only.

But if a high pressure engine could be made with equal security, would not that be more convenient to be used on board a boat than a condensing-engine?—It would take less room.

Would not it in many cases, as they are now constructed, consume less fuel in proportion to the power?—I am not acquainted with that fact; but I have frequently asked, and I find in the common high pressure engine there is no saving in the fuel, but they are cheaper and more simple in their construction.

Do you apply that to the high pressure engine which they call the Trevethick engine?—Yes.

Not to any other?—Not to Woolf's.

Nor to Simms's?—I have never seen either one or the other.

Suppose that a high pressure engine was to be used in a boat, what construction of boiler or safety-valve applied to that boiler should you advise, in order to give it the greatest possible security?—I really am unable to answer that question satisfactorily; of course the more safety-valves there are employed, the greater security there will be against the chance of explosion; I believe that the principal source of the explosion of high pressure engine

gine boilers of cast iron, arises from allowing them to get cool too suddenly, and raising the steam too suddenly, the metal contracts and expands at a period when we cannot investigate its occurrence.

Wrought iron would not be attended with that danger?—Not to the same extent; the rivets would go.

But not with the same degree of explosion?—No.

Would it not be a great safeguard in the construction of a boiler, if a safety-valve was so made as to be put out of the power of the engineer to get at it?—No doubt, it ought in all cases to be so.

It could be so constructed?—No doubt; if the pressure, however, is greater than what the safety-valve is intended to relieve, there might be an accident from the causes which I previously stated; that is, that a boiler might be defective without its being known.

You apply that to cast-iron boilers?—Yes, and in a small degree to wrought-iron boilers.

Do you conceive it impossible, or even difficult, to construct a wrought-metal boiler, with safety-valves properly adjusted to its capacity, and a mercurial gauge, supposing that to be capable of being applied, which should render a high pressure engine on board a steam-boat what might be called perfectly safe?—No, I do not think it impossible; and I hope some time or other it will be accomplished.

Wherein do you apprehend that the difficulty of so constructing a boiler would consist?—Because I have found that difficulty in making boilers myself steam-tight, even for condensing-engines, where the plates were of a thickness fit to undergo high pressure.

Do you apprehend that any danger of a fatal accident could arise from that mere want of tightness in the riveting, which would permit some steam to escape?—That danger would depend upon the degree of the pressure, and the extent of the aperture through which the steam escaped.

What is the species of danger which you would expect to occur in such a case?—I am not able to answer the extent of it. If the safety-valves acted, of course the danger would be removed; supposing that the safety-valves are properly constructed, and their operation is secured, the danger would not be extremely great; it is only from their defect of action that the danger is to be apprehended.

Then do you mean to say, that if the valves were really in point of fact performing their functions properly, in that event you would not consider there was any danger?—Certainly not, if the boiler was adequate to the pressure.

What is the pressure per inch which you conceive to be ge-

nerally used in the condensing-engine?—From two and a half to four pounds.

Do you not apprehend that the strength of the boiler is calculated upon what may be required to resist that low pressure?—Yes.

Is it not extremely possible, in the common use of a condensing-engine, that by accident, or the inattention of the engineer, the pressure may be increased very much beyond that which you have just mentioned?—No inattention would produce it while these securities exist; because the water would be discharged through the feed-pipe, and the mischief prevented.

Did you ever know the steam-pipe used in any condensing-engine on board a steam-boat?—I fancy they never are.

Did you not hear that the Norwich boiler was blown up by the very fact of the inattention or temerity of the engineer?—I did hear that.

Is not that inattention or temerity equally to be applied to a condensing as to a high pressure engine?—The engineer may tie down the valve occasionally; it is very natural to expect it in steam-boats. I fancy it is too frequently done; there are instances where something of that sort was said in conversation at Norwich, that where a man waited for passengers, and wanted to get up with the other boats, he did it.

Could a mercurial syphon be applied to a boiler, so that it would meet the observation of all the passengers on board the boat?—I should think it could; but the discharge of mercury, in case of explosion, might produce very serious effects.

If the syphon was of a sufficient bore, it would be the means of preventing the effects you have spoken of?—No doubt.

In order to give security to the public in travelling by steam-boats, do not you think that it might be necessary to have an examination of each engine two or three times in a year?—Certainly; once every six months. I think it would create confidence, and that is a great object.

And that is your opinion, whether the boiler is constructed of cast iron or wrought metal?—Yes.

You think that without this examination a condensing-engine would be unsafe?—I think it would be advantageous to have an examination.

In your judgement, would an inspection of the boilers of a steam-engine, of a condensing-engine, and a high pressure engine, be equally necessary, with a view to give security against accidents by explosion?—Yes; both.

Do you think any danger to lives is to be apprehended from condensing-engines without examination?—I do not think any material danger would arise.

[To be continued.]

XIII. *Further Considerations on the Doctrine that the Phænomena of Terrestrial Gravitation are occasioned by known Terrestrial Motions.* By Sir Richard Phillips.

To Mr. Tilloch.

Sir, — Since the publication of the theory which resolves the phænomena of weight, and of falling bodies, into the orbicular and rotary motions of the earth, objections have been started, by various persons in conversation, and through the public press—which the author's love of truth, and his respect for some of the parties, induce him to consider.

I. *It has been doubted whether bodies would fall in the exhausted receiver of an air-pump upon this hypothesis.*

To this it may be replied, that the exhausted receiver, the contained vacuum, and the bodies let fall before, and at the instant of fall, are all of them as much the patients of the orbicular and rotary motions, as though no such exhaustion had taken place. The orbicular motion was carrying forward the whole, and the rotary motion was endeavouring to deflect every part of the sustained mass, from the right line of the orbicular motion. The difference arising from the absence of the air is the same, whatever might be the source of the power which caused the bodies to fall; that is, a feather would fall in the same time as a guinea, simply because the atmosphere opposed no resistance, whether the centripetal force was produced by governing motions or by attraction.

II. *It is objected that a projectile would continue to ascend for ever, unless the force of attraction drew it towards the earth.*

To this I reply, that the deflective force of the rotary motion is equivalent, in the retardation of a projectile, to the supposed attraction; and that, in combination with the resistance of the atmosphere, that deflective force produces all the phænomena of projectiles, being the orbicular force common to both hypotheses.

III. *It has been objected that, if a body were let fall in the atmosphere, it would either go off in a tangent into space, or would move for ever in that place, but for the earth's attraction.*

In regard to the assertion, that it might move off in a tangent, it need only be considered, that no force has been given it in the direction of such tangent, and that bodies do not move in any required direction without some force exerted in that direction.

And that it will not move for ever in an unsupported spot in the atmosphere, arises from the influence of the deflecting rotary motion, of which it partook when placed there, in which it continues, and which it also derives from the surrounding medium.

 IV *It.*

IV. *It is contended that the Galilean laws of falling bodies cannot be accounted for, except on the principle of a continually acting attraction.*

To this I reply, that the great terrestrial motions are, in like manner, continually acting; and that from like causes they must produce like phænomena whenever any body is placed in circumstances to become the sensible patient of their activity.

V. *It is urged that local affections of mountains, or other masses, can result only from the attractive influence of those masses; and the experiments of Maskelyne and Hutton, of Bouguer, of Zach, and of Cavendish, are adduced as proofs.*

A mighty host, if their acumen and their accuracy bore on the question! But, as I refer all phænomena to a centre of motion, and the Newtonians refer them to a centre of attraction, and as both centres are generated by the actual dispositions of all the masses of the aggregate—so both centres are varied in position by unequal arrangements of the masses; and the motions on the surface referable to such centres are varied accordingly, and in equal degrees, upon both hypotheses.

If the earth were an equal and homogeneous sphere, then all the phænomena of falling or suspended bodies would have reference to the mathematical centre of the mass, and the plumb-line would always hang perpendicularly to the visible horizon; but, if a mountain, or any unequal mass, be placed on the surface, then on one hypothesis the centre of the motion, or on the other the centre of the attraction, will be raised above the mathematical centre, in a certain proportion, towards that mountain, creating a new physical centre; and all the deflections of the rotary motion on this theory, or all the attractions on the Newtonian theory, will be made with reference to that new centre. The maximum of variation will take place nearest to the projecting mass; and, if the mass were suddenly created, or brought near a suspended plummet, it would turn it aside, in a given proportion of the bulk of the mass to the bulk of the earth; and, as in Mr. Cavendish's experiment, it might perhaps be possible to measure the impulse. But, in every possible case of such inequalities, the same phænomena must and would result from thus varying the centre of the aggregate; whether the phænomena were ascribed, as now, to the efficient and operative motions of the earth, or, as heretofore, to the principle called by the name of attraction*.

VI. *It*

* I have taken it for granted that these experiments and calculations are correct, because the true results must be included in the laws of motion, as well as those of gravitation; but I remark, with profound deference to the learned calculators, that the Schihallien result assumes two-thirds of the circumference for the earth's attraction as a quantity *admitted;* and, in Mr.

VI. *It is urged, that, as attraction is admitted to produce certain phænomena in electricity, galvanism, chemistry, magnetism, and optics, so the attraction of gravitation is but an analogous power, and might, in like manner, be admitted.*

This argument, to say the least of it, is a very indirect one, and includes a large appeal to faith. I say again, and with little danger of refutation, that the terms attraction and gravitation were chimeras of the middle ages, growing out of the schools of astrology and magic; and, in the writings of the illustrious Newton, are akin to the ghosts of the equally illustrious Shakespeare, or to the sympathies which filled the heads of all philosophers in those days. They may be used like characters in an algebraic equation; but it is incorrect to substitute them for real quantities, or efficient causes, or to set them up in opposition to the operative powers of nature, when these are found to be sufficient to explain phenomena. Nothing, in truth, has tended more to retard the progress of science than thus stopping at the phænomena of attraction, and then impiously treating this secondary cause as the proximate effect of omnipotent agency, though it is found to act mechanically and subordinately, according to certain laws of the distance!

This is not the place to enter into details to prove that the different species of mechanical affection, without contact, must all be created by different actions of the affected bodies on the media which lie between them; or, mutually, on the surfaces of the bodies and the surfaces of the media. I confidently, however, calculate on the discovery of the *modus operandi* by which every species of attractive phænomena is effected, as among the probable triumphs of experimental philosophy. I, therefore, consider the argument in support of a terrestrial attraction, drawn from the analogy of supposed local attractions, as irrelevant, because, in the sense in which the terms are used, I believe that no attraction exists, and that in due time this term will give way in all the perfect sciences to its explanations or definitions.

VII. *It is objected that this illustration of the cause of terrestrial gravitation tends to overturn the Newtonian philosophy, which is built on the immutable bases of geometry.*

To this I reply, that as the great Newton did not affect to explain this cause, but merely admitted this name of the effect, so any hypothesis which seeks to account for it can have no neces-

Mr. Cavendish's leaden-balls' experiment, the earth's attraction is *assumed* to be represented by its diameter—that is, in both cases, a quantity unknown, and growing out of the hypothesis of gravity, is taken for granted to prove that very gravity. If the known bulk, force, and density, of the mountain and the balls were, by exact analogy, to be compared with the known bulk of the earth, to determine its force and density, then the results will be totally different, and the irrelevancy of the experiments be manifest.

sary opposition to his system. At the same time there is a latent, though popular error, in confounding physics and geometry, for all physical effects result from competent proximate causes, often *varying*; and all geometrical laws result from relations, always *fixed*. But, if our excellent philosopher so well accounted for the phænomena of the solar system by geometry, founded on the basis of an occult principle, with how much more satisfaction would he have done it on a mechanical basis! The author of this hypothesis has calculated, however, on no change but in nomenclature.

VIII. *It is asserted, that as gravitation is a fiat of Omnipotence, so to attempt to account for it is beyond the due bounds of philosophical inquiry.*

Without intending any personal disrespect to those who have used this argument, it may be asserted, that such has been the prejudice of ignorance from the age in which man first used a spade to augment the natural productions of the earth, to the days of Galileo, and even to our time, when Jenner discovered the means of extirpating a fatal disease. Shall we more nearly approach the CAUSE OF CAUSES in determining the mechanism by which a planet is held together, or by which a system moves, than by investigating the circulation of the blood, or by the chemical analysis of any substance in Nature? The causes of motion would still remain behind; and, were a future age to discover these, the prime mover of all things, the sublime and incomprehensible Creator and Preserver, would still be at an infinite distance from the finite powers of man.

IX. *It is asserted that the law of gravitation is not proved to be the law of motion.*

To prove the affirmative of this proposition was, however, the entire business of the "*Principia*" of Newton, and has been the employment of all mathematicians from his time to our own. If the laws of motion are not the laws of gravitation, then have philosophers been dreaming during the last hundred years. I merely identify what they have proved; and, as mathematicians have, by the hypothesis of gravitation, proved the laws of motion, I now desire to discard the unknown or assumed quantity, and to restore the known motions of Nature in its place—for the purpose of explaining the *modus operandi* by which the phænomena are produced.

It is imagined that I had forgotten the relations of radii and circles; I was not, however, alluding to circles, but to the surfaces of concentric spheres, which were the objects of discussion, and which are to each other as the *squares* of their radii. The *spaces* generated on *spherical surfaces* being to each other as the squares of their radii, it follows that the *quantities* of motion generated

generated in each stratum, and the *forces* generating those motions, are in the same ratio. On this point there is nothing to add or to alter. If the concentric strata were in density reciprocally as the squares of their distances, and undisturbed, there would be no phænomena; but it is the disturbance of that which has been in a state of equilibrium (either by distance from the centre, or by the resistance of friction), which occasions the sensible phænomena of weight, or of falling bodies.

I do not, however, consider that these observations conclude the subject; for I admit, that all the circumstances which exist among the parts of a sphere, moving in an orbit, the momenta of whose masses in the concentric strata are equalized by a rotary motion, as well as the effects arising from the centre of density, not being the mathematical centre; and also from accidental disturbances in the equilibrium of particular bodies, merit the careful analysis of philosopical mathematicians.

At the same time, although the mathematical laws must necessarily be the same, it is not indifferent, in human inquiries, whether physical phænomena are ascribed generally to gravitation, of which nothing is affected to be known, or to motion, of which we may not know the primary origin. We know, at any rate, more of motion than we know of gravitation. Besides the laws common to both, we know that motion is an accident of bodies which gives them momenta, and causes them to change their situations in space; and we know that some motions are general, antecedent, or primary, and that others are local, consequent, or subordinate. In the problem before us, we are therefore enabled to show that known effects are consequences of several known motions, thereby attaining a degree of analysis, which could never be effected, if we referred the same phænomena to the general name of gravitation.

Conclusion. These, I believe, are the chief objections which have been imagined and promulgated in opposition to a theory which substitutes the known motions of Nature as operative causes of certain physical phænomena, in place of an assumed principle called gravitation, by which, false analogies have been introduced into philosophy, and effects ascribed to a cause neither proximate nor in contact. It may be difficult to analyse, in like manner, the motions which produce all the celestial phænomena, or trace the sources of particular motions; and it may be impossible for man to ascertain any other origin of motion than the sublime CAUSE OF CAUSES: but we advance another step in human knowledge when we discover that the two-fold motions of a planet are competent to the consolidation and unity of its mass, and are efficient causes, by means of which bodies removed out of their equilibrium are restored to the mass.

XIV. *On*

XIV. *On the Oxi-hydrogen Blow-pipe.* *By Mr.* Robert Hare, *of Philadelphia.*

To Mr. Tilloch.

Philadelphia, June 27, 1817.

Sir,—It is now almost fourteen years since you honoured my memoir on the Supply and Application of the Blow-pipe with a place in the Philosophical Magazine, vol. xiv. In that paper it will be seen that the heat, produced by the ignition of the gaseous elements of water, was employed by me in 1801–2, in fusing or volatilizing the most refractory earths and metals. A subsequent article, in the sixth volume of the American Philosophical Transactions, mentions the fusion of strontites, and complete and rapid volatilization of platinum. Yet Dr. Clarke has lately published a paper on this subject, as if it were an original discovery. I therefore inclose you a memoir of my friend Professor Silliman, by which it will be seen how far Dr. Clarke can be justified for treating his experiments as new. I hope you will republish it. It is a simple act of justice, which I should hope, as the editor of a scientific journal, you will render me without hesitation. I request any fellow-labourer in the laboratory to reflect on the injustice, which is likely to be done, to Professor Silliman and myself, in having the facts mentioned by Dr. Clarke as his own, quoted on his authority instead of ours.

I am, sir, with due consideration,

Your obedient servant,

Robert Hare.

Experiments on the Fusion of various refractory Bodies, by the Compound Blow-pipe of Mr. Hare. *By* Benjamin Silliman, *Prof. Chem. and Min. in Yale-College* *.

A section of the *Pneumatic Cistern* of Yale College, with the *Compound Blow-pipe* of Mr. Hare for burning Hydrogen mingled with Oxygen Gas, is shown in fig. 1. (Plate II.)

References to the Figure.

AAAA.—The pneumatic cistern, filled with water. For a plate and full description, see the Boston edition of Henry's Chemistry.

B.—A gas reservoir, of the capacity of twelve gallons, filled with oxygen gas, either by the action of the hydrostatic bellows at O, or by a recurved tube passing from above through the water, and hooked under B: parallel and contiguous to B, on

* From Memoirs of the Connecticut Academy of Arts and Sciences, vol. i. part iii. 1813.

the

the other side of the cistern, is another gas reservoir, of the same capacity, which may be connected with B, or not, at pleasure.

C.—The same, in every respect; only C is filled with hydrogen by hydrostatic bellows at OO, or by a recurved tube, as above.

D.—Copper tubes, half an inch in diameter, furnished with stop-cocks at *f*, and inserted into the gas reservoirs B,C.

E.—Recurved tubes of flexible metal, furnished with double screws at F, which connect them with a pair of brass blow-pipes, cut off at G, and soldered to two strong cast silver tubes, which screw, *air-tight*, into H, an inverted pyramidal piece of platinum, in which two converging ducts as large as a pin are perforated, forming a continuation of the tubes, and uniting in a common passage, somewhat larger, just before their exit, at the common orifice below. The subject to be operated upon is sustained by charcoal, or forceps, and held by the hand, just below the orifice in the piece H.

The gases at BC are under hydrostatic pressure, which is easily recruited as the gases run out, either by throwing common air with the bellows into one of the spare reservoirs, or by introducing more of either of the gases into the appropriate reservoir, and peculiarly of hydrogen, both on account of the facility with which it is obtained, and because twice as much of it, in bulk, is wanted as of oxygen.

The rapidity of efflux of the gases, and their due proportion, are easily regulated, by turning, more or less, the keys of the stop-cocks at *f*; and the effects of either gas alone, may be observed by shutting the stop-cock leading to the other.

When the compound flame is desired, the hydrogen is first let out and fired; the blaze should be somewhat larger than that of a candle; the oxygen is then let into the hydrogen till the effect is the greatest, which a little habit will soon ascertain.

The flame of the hydrogen is very much narrowed by the introduction of oxygen, and there is no appearance of peculiar splendour or heat, till some body capable of reflecting the light and heat is placed in the focus, which is usually about one-fourth of an inch below the orifice.

All the apparatus below FF is easily detached by turning the double screws;—the strong silver tubes are intended to prevent fusion of this part of the apparatus, and to admit of connexion with the platinum piece by *means of a screw cut on the silver tubes;* this obviates the necessity of using a solder, which would be very liable to melt, and the platinum piece is, for a similar reason, substituted for the silver cylinder originally used by Mr. Hare, as experience has shown that these are liable to fusion.

No

No flux or addition of any kind was employed in the following experiments.

On the Fusion of various refractory Bodies by the Compound Blow-pipe of Mr. Hare.

The philosophical world behold with pleasure and astonishment the effects produced on the fusion and combustion of bodies by a stream of oxygen gas directed upon burning charcoal. The splendour of these experiments arrested universal attention; and Lavoisier, with his gasometer, was enabled in this manner to produce a degree of heat surpassing that of the most powerful furnaces, and even of the solar focus. Bodies which no degree of heat, previously applied, had been able to soften, now became fluid, and philosophy appeared to have attained the limit of its power in exciting heat; indeed, it seemed to have advanced very far towards realizing the opinion, that solidity and fluidity are accidental attributes of bodies, dependant solely on the quantity of caloric which they contain, and that therefore they may be supposed capable of existing in either of these conditions.

Still, however, there were, *in fact*, many important exceptions. Of the primitive earths, Lavoisier had been enabled to fuse only alumine—while the rest remained refractory, and seemed fully entitled to the character of infusibility, usually attributed to this class of bodies: many native minerals, and especially those which are most distinguished for hardness, beauty, and simplicity of composition, maintained the same character, and some of them refused to melt even when heated with powerful fluxes.

The beautiful invention of Mr. Robert Hare of Philadelphia, by which he succeeded in burning, with safety and convenience, the united stream of oxygen and hydrogen gases, greatly extended our dominion over refractory bodies, and presented new and very interesting results. Mr. Hare's memoir, originally communicated to the Chemical Society of Philadelphia, has been some years before the public, and has been republished and handsomely noticed both in France and England. Still, however, his results have not found their way into the systematical books on chemistry, (with the exception of Mr. Murray's system,) notwithstanding that some of the European Professors have availed themselves of Mr. Hare's invention, so far as to exhibit his most splendid and striking experiments to their classes.

The writer of this article, although fully disclaiming any share in Mr. Hare's invention, was early associated with him in his experiments: they excited in his mind a degree of interest, which led him to hope that they would be repeated and extended by others; but as nothing of this kind has appeared in this country, perhaps

perhaps the following experiments may not be altogether uninteresting, especially as they were performed with an apparatus of a construction somewhat more simple than the original.

It will be necessary to recollect that Mr. Hare not only melted alumine, which Lavoisier had done before, but also *silex* and *barytes*, and by subsequent experiments he added *strontites* to the list of fusible bodies: he was inclined to believe that he had volatilized gold and silver, a conclusion which was rendered highly probable by his having afterwards evidently volatilized platinum.

The experiments of Mr. Hare, as will appear below, have been repeated by the writer of this paper with success; and many other bodies among the most refractory in Nature have been melted. For the sake of showing how far the experiments now to be recited have affected our knowledge of the dominion of heat, quotations, for comparison, will occasionally be made from one of the latest and most respectable chemical authorities—Murray's System, 2d ed.

Bodies submitted to the Heat of the Compound Blow-pipe of Mr. Hare.

PRIMITIVE EARTHS.

Silex—being in a fine powder, it was blown away by the current of gas; but when moistened with water it became agglutinated by the heat, and was then perfectly fused into a colourless glass.

Alumine—perfectly fused into a milk-white enamel.

Barytes—fused immediately, with intumescence, owing to water, as observed by Lavoisier; it then became solid and dry, but soon melted again into a perfect globule, a grayish-white enamel.

Strontites—the same.

Glucine—perfectly fused into a white enamel.

Zircon—the same.

Lime—in small pieces, it was immediately blown off from the charcoal: to prevent this, as well as to obviate the suspicion that any foreign matter had contributed to its fusion, the following expedient was resorted to. A piece of lime, from the Carrara marble, was strongly ignited in a covered platinum crucible; one angle of it was then shaped into a small cylinder, about one-fourth of an inch high, and somewhat thicker than a great pin: the cylinder remained in connexion with the piece of lime: this was held by a pair of forceps, and thus the small cylinder of lime was brought into contact with the heat, without danger of being blown away, and without a possibility of contamination: there was this further advantage, (as the experiment

was

was delicate and the determination of the result might be difficult,) that, as the cylinder was held in a perpendicular position, if the lime did really melt, the column must sink and become, at least to a degree, blended with the supporting mass of lime. When the compound flame fell upon the lime, the splendour of the light was perfectly insupportable by the naked eye; and when viewed through deep-coloured glasses (as indeed all these experiments ought to be), the lime was seen to become rounded at the angles, and gradually to sink, till in the course of a few seconds only a small globular protuberance remained, and the mass of supporting lime was also superficially fused at the base of the column, through a space of half an inch in diameter. The protuberance, as well as the contiguous portion of lime, was converted into a perfectly white and glistening enamel; a magnifying glass discovered a few minute pores, but not the slightest earthy appearance. This experiment was repeated several times, and with uniform success; may not lime therefore be added to the list of fusible bodies?

Magnesia.—The same circumstances that rendered the operating upon lime difficult, existed in a still greater degree, with respect to magnesia; its lightness and pulverulent form rendered it impossible to confine it for a moment upon the charcoal; and as it has very little cohesion, it could not be shaped by the knife, as the lime had been. After being calcined, at full ignition, in a covered platinum crucible, it was kneaded with water, till it became of the consistence of dough. It was then shaped into a rude cone as acute as might be, but still very blunt; the cone was three-fourths of an inch long, and was supported upon a coiled wire.

The magnesia thus prepared, was exposed to the compound flame: the escape of the water caused the vertex of the cone to fly off in repeated flakes, and the top of the frustum, that thus remained, gave nearly as powerful a reflection of light as the lime had done: from the bulk of the piece (it being now one-fourth of an inch in diameter at the part where the flame was applied) no perceptible sinking could be expected. After a few seconds, the piece being examined with a magnifying glass, no roughnesses or earthy particles could be perceived on the spot, but a number of glassy, smooth protuberances, whose surface was a perfectly white enamel. This experiment was repeated with the same success. May not magnesia, then, be also added to the table of fusible bodies?

Yttria—was the only remaining primitive earth; but no specimen of it could be obtained.

Perhaps then we shall be justified in saying, in future, that the primitive earths are fusible bodies, although not fusible in furnaces,

naces, in the solar focus, nor (with the exception of alumine, and possibly barytes,) even by a stream of oxygen gas directed upon burning charcoal.

Platinum—was not only melted, but volatized with strong ebullition.

VARIOUS MINERALS.

Rock Crystal—transparent and colourless. This mineral was instantly melted into a beautiful white glass. "It not only does not melt in the focus of the most powerful burning mirror, but it remains without fusion, at least when in the state of rock crystal, in the still more intense heat excited by a stream of oxygen gas directed on burning charcoal." (Murray, ii. 261.) . "It is even imperfectly softened by the intense heat, excited by a stream of oxygen gas, directed on the flame." (of the blow-pipe lamp.) —(Ibid. iii. 518.)

Common Quartz—fused immediately into a vitreous globule.

Gun Flint—melted with equal rapidity: it first became white, and the fusion was attended with ebullition and a separation of numerous small ignited globules which seemed to burn away as they rolled out of the current of flame: the product of this fusion was a beautiful splendid enamel.—"It is infusible before the blow-pipe, but loses its colour."—(Ibid. 518.)

Chalcedony—melted rapidly, and gave a beautiful blueish-white enamel resembling opal. "It is infusible before the blow-pipe." —(Ibid. 516.)

Oriental Carnelian—fused with ebullition,* and produced a semitransparent white globule with a fine lustre.

Red Jasper—from the Grampians, was slowly fused with a sluggish effervescence; it gave a grayish black slag, with white spots.

"It is infusible before the blow-pipe, even when the flame is excited by a stream of oxygen gas."—(Ibid. 519.)

Smoky Quartz—or smoky topaz melted into a colourless globule.

Beryl—melted instantly into a perfect globule, and continued in a violent ebullition as long as the flame was applied; and when, after the globule became cold, it was heated again, the ebullition was equally renewed; the globule was a glass of a beautiful blueish-milky white.

"The beryl is melted with difficulty before the blow-pipe alone, but easily when borax is added."—(Ibid. 511.)

Emerald of Peru.—The same; only the globule was green, and perfectly transparent.

Olivin—fused into a dark-brown globule, almost black. "It can scarcely be melted by the blow-pipe without addition."—(Ibid. 534.)

Vesuvian

Vesuvian—instantly melted into a beautiful green glass. "It melts before the blow-pipe into a yellowish glass."—(Ibid. 534.)

Leucite—instantly fused into a perfectly transparent white glass: the fusion was attended with strong ebullition, and many ignited globules darted from it and burnt in the air, or rolled out upon the charcoal and then burned. Were they not potassium? This stone contains full 20 per cent. of potash;—This hint will be resumed below.

"It is not fused before the blow-pipe."—(Murray, iii. 534.)

Chrysoberyl—(Cymophane of Haüy) was immediately fused into a grayish-white globule. "It is not melted by the blow-pipe."—(Ibid. 499.)

A crystallized Mineral.—From Haddam, Connecticut; according to the Abbe Haüy, it is *chrysoberyl*; according to Colonel Gibbs, *corundum*: it fused with ebullition and scintillations, and produced a very dark globule almost black.

Topaz—of Saxony, melted with strong ebullition, and became a white enamel. "It is infusible before the blow-pipe, but melts when borax is added."—(Ibid. 498.)

Sappar or *Kyanite*—perfectly and instantly fused, with ebullition, into a white enamel. "It remains perfectly unaltered before the flame of the blow-pipe even when excited by oxygen gas."—(Ibid. 499.)

Corundum—of the East Indies, was immediately and perfectly fused into a gray globule.

Corundum—of China, the same with active ebullition. *Corundum* "is not fused by the flame of the blow-pipe on charcoal even when soda or borax is added to it."—(Ibid. 495.)

Zircon—of Ceylon, melted with ebullition into a white enamel. "It is not melted alone before the flame of the blow-pipe, but if borax is added it forms a transparent glass."—(Murray, iii. 539.)

Hyacinth—of Expailly, fused into a white enamel. "It loses its colour before the flame of the blow-pipe, but it is not fused; it melts with borax into a transparent glass."—(Ibid. 540.)

Cinnamon stone—instantly fused into a black globule with violent ebullition.

Spinelle Ruby—fused immediately into an elliptical red globule. "It does not melt before the blow-pipe, but is fused by the aid of borax."—(Ibid. 497.)

Steatite—melted with strong ebullition into a grayish slag.—"It does not melt before the blow-pipe, but becomes white and very hard."—(Ibid. 482.)

Porcelain, common pottery, fragments of Hessian crucibles, Wedgwood's ware, various natural clays, as pipe and porcelain clay,

clay, fire and common brick, and compound rocks, &c. were fused with equal ease.

During the action of the compound flame upon the alkaline earths, provided they were supported by charcoal, distinct globules often rolled and darted out from the ignited mass, and burnt, sometimes vividly, and with peculiarly coloured flame. From the nature of the experiments, it will not be easy to prove that these globules were the basis of the earths, and yet there is the strongest reason to believe it; circumstances could scarcely be devised more favourable to the simultaneous fusion and decomposition of these bodies; charcoal highly ignited for a support, and an atmosphere of hydrogen also in vivid and intense ignition; that the oxygen should be, under these circumstances, detached, is not surprising; but the high degree of heat and the presence of oxygen necessarily burn up the metalloids almost as soon as produced. If means could be devised to obviate this difficulty, the blow-pipe of Mr. Hare might become an important instrument of analytical research.

We can scarcely fail to attribute some of the appearances, during the fusion of the leucite, to the decomposition of the potash it contains.

This impression was much strengthened by exposing potash and soda to the compound flame, with a support of charcoal; they were evidently decomposed: numerous distinct globules rolled out from them, and burnt with the peculiar vivid white light and flash which these metalloids exhibit when produced and ignited in the Galvanic circuit. It is hoped that these hints may induce a further investigation of this subject.

The experiments which have now been related in connexion with the original ones of Mr. Hare, sufficiently show that science is not a little indebted to that gentleman for his ingenious and beautiful invention.—It was certainly a happy thought, and the result of very philosophical views of combustion, to suppose that a highly combustible *gaseous* body, by intimate mixture with oxygen gas, must, when kindled, produce intense heat: and it is no doubt to this capability of perfectly intimate mixture between these two bodies, that the effects of the compound blow-pipe are in a great measure to be ascribed.

This communication has already been extended further than was contemplated; but on concluding it, it may be allowable to remark, that there is now in all probability no body, except some of the combustible ones, which is exempt from the law of fusion by heat. If the primitive earths, and such minerals as several of those which have been mentioned above, are fusible, no doubt can be entertained that all other mixtures and combinations of earths are fusible also: for such mixtures and combinations are

known to be more fusible than the primitive earths; the metals are more fusible than the earths; and the diamond, along with carbon in its other purest forms, appears to be really the only exception; and it is probable that this is only a *seeming one*, for it is scarcely possible to expose these bodies to the heat of the compound blow-pipe, without at the same time burning them up: could the heat be applied without exposing them to the contact of oxygen, is it not probable that they also would be added to the list of fusible bodies?

Yale-College, May 7, 1812.

To the foregoing (which has been printed from the published Transactions of the Connecticut Academy) the following P. S. was added in manuscript:

"P. S.—In subsequent experiments gold, silver, platina, and most of the metals were not only volatilized but burnt with pe-culiar flames."

Some of my readers may be inclined to think that the facts do not warrant all that Mr. Hare has stated respecting Dr. Clarke's claim as an inventor. On that point I shall give no opinion; but it should be observed that in Mr. Hare's apparatus the gases are not in mixture till they are brought together at the piece H.

By Mr. Hare's arrangement it is obvious that the operator is completely secured against any danger from an explosion; and it must appear equally obvious to any person who shall consider the subject, that by having two condensing vessels for the gas-reservoirs B and C, every result can be obtained which the united gases from one vessel can possibly yield: for, by means of the cocks at *f* the efflux of the gases may be regulated, as remarked by Professor Silliman, till any required degree of mixture or effect is produced.—A. T.

XV. *On the Steam-Vessel proposed to be employed between London and Exeter.* By A CORRESPONDENT.

To Mr. Tilloch.

SIR,—HAVING been long of opinion that vessels propelled by steam may be used with advantage for the general coasting trade; I have at length determined, in conjunction with some friends who are of the same opinion, forthwith to establish a vessel for conveyance of goods and passengers from London to this port. The particular advantage such an establishment would have here, over vessels of the ordinary description, is the degree of certainty attending the setting out and arrival; the want of which in

in the usual mode of water-carriage induces many tradesmen to have goods by that more expensive conveyance, the waggon; and others are subject to much inconvenience by having their goods detained for a month or six weeks, by the prevalence of westerly and south-westerly winds. The only steam-vessels I have had an opportunity of seeing are those used on the Thames, which being constructed for passengers chiefly, are not adapted for goods, being deficient of stowage:—besides, from their having such an extended width of deck, to cover the paddle-wheels, it is conceived these vessels, or any enlarged vessel on such a model, would be unsafe in the Channel. I have therefore subjoined a sketch of the plan on which it is proposed to construct our vessel (see Plate II. figs. 3 and 4). The form intended is that which may be most approved for stowage and sailing, or rather that will move through the water with the least resistance. The paddle-wheels are proposed to be placed at the stern, for the following reasons. First, to obviate the inconvenience of the increase of deck by their being placed over the sides. Secondly, by placing them at the stern, the diameter or breadth of the wheels can be much increased without causing the roll such ponderous weights would occasion when on the sides. Thirdly, the machinery and boiler occupying the aftermost part of the vessel, will not interfere with so much valuable stowage. Fourthly, the vessel will lie by the side of a wharf for the purposes of taking in and discharging her cargo without injury to the wheels. And lastly,—and which for this port, is a matter of more importance than the preceding, having to pass through two locks of a navigable river,—the paddle-wheels must otherwise be contrived to take off, to admit the vessel through.—My object in this address is to gain the opinions of your more experienced readers, and to adopt such hints as may be gathered from others who have made this subject their study.

I am, sir,

Your most humble servant,

Exeter, July 29, 1817. C.

XVI. *A Mathematical Question.* *By* A Correspondent.

To Mr. Tilloch.

Sir, — Considering, how many able Mathematicians read your truly scientific Work, and often correspond with you, it has somewhat surprised me, that so few of them have appeared to notice, and take a part in the elaborate and curious researches, relating to Musical Intervals, which have been occasionally sent to you for insertion, in your last 24 volumes, by *Mr. Farey sen.*

 within

within the period of the last 10 years; particularly since the publication of Mr. Liston's Essay, in 1812, and since the frequent exhibition of his Euharmonic Organs, have given those experimental and practical illustrations on the subject, which before were a good deal wanting.

In the hopes of obtaining answers from several of your Correspondents alluded to, I beg to propose to them the following Question: which I have been enabled myself to solve, principally, through studying the paper inserted in your last volume, p. 442; viz.

What are the *Ratios*, *Values* (in Mr. Farey's Notation), the *Names*, the *Vibrations* and the *Beats* in 1″ of the three following Intervals, above Tenor Cliff C, viz. $G'''b^{19}$ $F''b4$, and $B''*$?

I am, your obedient servant,

July 26, 1817. PHILO-MUSICUS.

P. S. It is a good rule, which I have observed Mr. Farey and other correct Writers follow, of always (or mostly) defining or expressing Musical Intervals, *in more than one mode*, for avoiding mistakes or ambiguity, through errors of the press, or misconception. I will therefore here, although the *literal* designation of the three required notes which are given above, are sufficient to determine them; further mention, that their ranges or places, i na sufficiently extended Listonian Tuning Table are, —12III —84V, +2III—37V, and III+8V, respectively: and I beg to add to my Question above proposed, the further request, that the answers thereto may, mathematically deduce these latter or *tuneable* definitions of the Intervals, from their literal ones.

XVII. *On the Cases of Injustice which Authors sometimes suffer from other Writers, and from Annotators; particularly the late Mr.* JOHN WILLIAMS, *Author of the "Mineral Kingdom." By* A CORRESPONDENT.

To Mr. Tilloch.

SIR,—YOUR pages, and those of every other independent periodical Journal, contain frequent instances of living Authors, seeing just occasion of complaint, on the score of injustice done to their literary labours, by other more recent Writers; and sometimes also, these complaints are either preferred or seconded by others, who have a personal friendship, or else a similarity of thinking and feeling, with the writer agrieved: and not unfrequently, persons are seen standing forward as the advocates of the reputation of Authors who are deceased, in cases where manifest

fest injustice may have been done or attempted, on the character of works, whose Authors or their personal friends, can no longer defend their writings, either as to the knowledge possessed by the writers, or, as to the honesty, ability or care, with which their sentiments were given.

In most instances, a feeling towards the support of undefended merit, as last mentioned, is sufficiently strong, to counteract and expose the improper designs or conduct of recent writers; but a case sometimes occurs, in which a person, not professedly a literary character, composes a Work, towards the close of his life, containing the results of his own laborious researches and experience, including perhaps, those of some of his friends also, in some practicable art or useful science, the details and principles of which he may have gone further in developing, than was current at the time, among the professed writers and Book-makers, who were his contemporaries, and immediate successors: and perhaps this person, may happen also, to adopt the expedient, at all times a hazardous one, of being the publisher of his own work, by subscription, without transferring to a regular publishing and advertizing Bookseller, any permanent interest in its literary success or general sale: in such last case, it is not uncommon, that the Writer should be able to print and give circulation only to a limited number of copies, just sufficient to make his work somewhat known, and began to be inquired after, when the Author is deceased and the work *out of print*, as is said, and no longer to be procured, but accidentally in the shops of second-hand Booksellers.

After a period of frequent inquiries for a book of the above description, it happens that some publishing Bookseller, with or without the knowledge or concurrence of the surviving relatives of the deceased Author, if he has any, entertains the design, of printing a new Edition of the Book, which seems thus in request; and in order to secure the chance of a more extended sale, instead of searching for any surviving friends of the deceased, who may be engaged in the same line of pursuit, or other persons practised therein, who could supply the illustrative notes or additions, which the further progress of knowledge since the first printing, may have shown to be necessary, in the opinions of such persons, as were fully conversant in the practical pursuits and views of the Author, and had visited the places he may have locally described, and attempting no further alteration of his work:—more probably, some learned Professor is sought for and engaged, in order to give *éclat* to the matter, by his own splendid additions to the new and *revised* Edition: these additions being perhaps, of a kind, very different from, and very inferior perhaps, in point of

consistency or practicable utility, with the original work, into which they were thus to be foisted.

In this way it is plain, that the generality of the readers and approvers of a Work, thus at second-hand, the original of which they may have never seen, may have but inadequate notions, of the real and comparative knowledge and merits of the deceased Author, and his original work, and are thereby prevented from discovering the full amount of injustice which the Annotator or others, may have done the Author; and under these circumstances, considerable time may elapse, before any one stands forwards, in such works as yours, or otherwise, to vindicate the deceased Author's credit, and put the public more fully in possession of the results of his labours

I have thus far spoken *generally*, in order now to attempt to apply a good deal of what has been said, to the case of the [illegible] Mr. JOHN WILLIAMS, a *practical* Miner and Collier, who in 1789, put to press in Edinburgh, near to which city he then resided, the result of more than 40 years' experience in his profession, and of unwearied research and inquiry, as to the Geological facts of almost every part of the British Islands, &c. under the title of "The *Natural History* of the MINERAL KINGDOM," &c. in two volumes, octavo.

Mr. Williams did not in his day, any more than a great part of the *practical* Miners, Colliers and Geologists of the present day, see, that any great good could result, from going into the nice technical distinctions of Minerals, under a very great variety of genera and species, far beyond the purposes of useful Geology or practical Mining (which Mineralogical *refinements* were beginning to be fashionable about the time he wrote, and have since greatly increased); such as could repay him for the labour and research necessary for making these distinctions, or for the diversion from his ordinary pursuits, of more practicable and useful kinds, which such an application to technical mineralogy would have occasioned.

Accordingly, most of those who have expected to find in Mr. Williams's Book, announced as above, any thing like *a mineralogical System*, or laboured technical *descriptions of Minerals*, much less a Geological System *founded on nice Mineral distinctions*, have been somewhat disappointed: the end and aim of the Author, having been very different, viz. that of detailing in plain and simple language, the chief phænomena of the Earth, regarding its *Strata* (those accompanying Coals in particular) their contortions, dislocations, and interesting Veins (those containing Metallic Ores in particular) Mountains, Volcanoes, &c. &c.

In the year 1810, a second Edition of Mr. Williams's Book

was

was printed in Edinburgh, "with *an Appendix* containing *a more extended view* of Mineralogy and Geology, by James Millar, M.D. F.S.A.S. Lecturer," &c. It appears from Dr. Millar's preface, that it was his intention at commencing the work, and until near 50 pages of it were printed off, "to add explanatory Notes to the original text of the Author;" but then the plan was changed, into that of giving Mr. Williams's Work without comment or illustration, merely divided in a more formal manner, into Chapters, and *curtailed of its redundancies;* and the appendix, as a separate work *by Dr. Millar*, was to be so enlarged, as to occupy all but the first 67 pages of the second volume: making in fact, two distinct works "independent, so that each may be perused as a whole," yet thus tacked together, rather too much in the Book-making style*.

In several careful perusals which I have given this second edi- of Mr. Williams's Work, in order to comprehend and treasure up the rich collection of *practical facts* which he has mentioned, and the many sagacious hints and suggestions which he gives, on the objects of my favourite study and pursuit, I have increasingly on every re-perusal, seen reason, to disapprove *the*

* In 1802, a Writer in England, *Mr. John Mawe*, eked out a meagre Octavo, entitled "The Mineralogy of Derbyshire," by 24 pages, of what he calls "An Analysis of Mr. Williams's Work, entitled The Mineral Kingdom;" on the frequent perusal of which "account of Mr. Williams's Book," I am unable to discover, any other motives or design the Writer had therein, beyond those hinted at in the text, and to exult in his own *assumed superiority*, as a technical Mineralogist, (or describer of, and *dealer in, hand Specimens*), and to abuse Mr. Williams, most unmercifully and unjustly, on the score of confusion, and tedious prolixity in his Ideas and Writings; in doing which, he has had the audacity to allege, at p. 178, that Mr. Williams's "real facts and observations," "are buried in a mass of idle declamation!;" again, in p. 184, that "nothing can exceed the prolixity of his declamations," "which rarely present one ray of solid information!!;" &c. &c.

For such conduct as this, towards his Author, it might have been expected by impartial and unprejudiced persons, that the Editor of Mr. Williams's 2d Edition, would therein have administered, due castigation to Mr. M.; that he would, on no account have omitted, *by notes*, on the 6 or 7 passages (at the most, in Mr. Williams's copious details) in which Mr. Mawe has expressly *contradicted any of the facts*, stated by Mr. W. to have vindicated him (as in justice he might, on most of them I believe), and to have properly explained Mr. W's excusable mistakes, on the others; such for instance, as the mighty fault of saying, the granite of Strontian was *gray*, instead of *red!*, &c.: it is too evident however to me, that this was not done, because Mr. M. and Dr. M., both entered on the critical examination of Mr. W's Work, without sufficient real, or *practical* knowledge, of most of the objects on which Mr. W. had expressly written; and having very similar feelings and intentions, each to raise their own reputation and sell their Books, almost regardless of the injustice thereby done, to the memory of Mr. W., or to the cause of scientific truth and improvement.

tone of superiority which his Annotator, as above mentioned, seems to assume, over Mr. W. in his various remarks, scattered through the work; calculated, too evidently, for lessening the Reader's estimation for Mr. W's knowledge and performance, and exalting that of the Annotator's own Appendix: which should have formed a separate publication, and stood on its own ground, not on the shoulders of Mr. Williams, as at present.

I do not wish to be understood herein, as entirely undervaluing Dr. M's performance, which certainly presents a very copious and useful *collection of extracts*, of great part of what has been detailed or written by Geological observers, in the Transactions of Learned Societies, and in other works of recent date, with many of the Doctor's own observations, the whole under such an arrangement, as would have done him credit in a separate publition, and been, perhaps, in every way commendable: but in their present situation, the great display made, of the technical knowledge of *Minerals in general* (the greater part of which, from their scarcity, are quite *unimportant in a practical point of view*, such as Mr. Williams professed to take) and the almost entire absence of proper illustrations, of the obscurities and defects of Mr. Williams's text, on the score of technical Mineralogical knowledge, which are so often alluded to, have certainly appeared to me, as improper, and have done so to many admirers of Mr. Williams's work, with whom I am acquainted.

It was not until very lately, although frequently inquiring since the year 1801, that I could procure a copy of Mr. Williams's first Edition, from which, and other circumstances I judge, that they must be very scarce. On perusing this copy, my opinion of the impropriety of Dr. M. as the annotator of Mr. Williams's Work, has been considerably strengthened, by observing, that the whole of *Mr. Williams's Preface*, containing 62 pages of curious and important matter, has been suppressed by Dr. M.!

What renders this omission the more questionable in its character, is, that although Dr. Millar, like many others of the modern partizans in Geology (who are alluded to with just reprehension by one of your Correspondents, in the Note in p. 47 of your last number) in pp. 560 and 565 of his Appendix, gives the outlines of *Dr. Hutton's* and *Mr. Werner's* Theories, and intimates, that these, *divide the opinions* of modern Geologists: yet he says not a word, that Mr. Williams, on whom he had obtruded himself as an Annotator, had examined Dr. Hutton's Theory, when recently published, and after his own work was ready for the Printer, and had in the latter 40 pages of his Preface (which Dr. M. had suppressed) considered and pointedly refuted, most of the leading tenets of this System!

If it should be stated in excuse of Dr. M's conduct herein towards

wards Mr. Williams, and towards Geological truth and impartiality, that his wish was *to exclude controversy*, and merely state facts and opinions, for the reader's own decision; then it should be rejoined, that he ought to have considered, what he has denominated chapter 3 of vol. i. (pp. 449 to 467), as among the useless "redundancies" of Mr. Williams's Work, because expressly employed, in considering and refuting the Theory of Count Buffon; in the same manner and on the same principles, as the refutation of Dr. Hutton's Theory, which has thus unfairly been kept back.

Conceiving, sir, that many of your Geological Readers, who possess Williams's 2d Edition, would wish to see the suppressed Preface to which I have been alluding, circulated and preserved in your pages, in portions, as room from more important matter may allow, I have sent you the Preface, and in case you oblige me, by its insertion, I propose to send you, occasionally, some further particulars, calculated to set Mr. Williams's labours and his work, in favourable points of view; and am,

Your obedient servant,

Aug. 4, 1817. AN ENGINEER.

[The Preface of Mr. Williams, referred to in the preceding article, will be given in subsequent Numbers.]—EDITOR.

XVIII. *On Vegetation in artificial Media.* *By Mr.* J. ACTON.

To Mr. Tilloch.

SIR, — OBSERVING some experiments in your Magazine for last month, by Mr. J. Tatum, respecting the effects of vegetation on the atmosphere, I beg to call his attention to some of my own, made several years ago, with the view of pointing out the analogy between the germination of seeds, the vegetation of plants, and the respiration of animals; as also of examining a new theory of the formation of carbonic acid gas in peculiar situations by seeds, plants and animals, then lately published by Mr. Ellis in a small octavo volume; and which experiments were published in the late Mr. Nicholson's Journal for July and October 1809. Although my subsequent experience has not led me to make any alteration in the conclusions to which my labours at that time led me, I cannot help feeling considerable diffidence with respect to them, from having since been compelled by many unfortunate circumstances to relinquish in a great degree pursuits so interesting and congenial to the inquiring mind. I had pledged myself to follow up those experiments by others particularly relating to vegetation, more effectually

effectually to have corroborated those already attempted; and which pledge I had the sincerest intention of redeeming, had I not found an absolute necessity for directing all the power of my faculties into other less important but more profitable channels. But at all events, as there appears to be a disposition evinced by Mr. Tatum to pursue these inquiries, I consider it my duty to point out to him what has been already done—not so much under the idea of my experiments being of sufficient consequence to supersede his investigations, as of their being perhaps worthy to be considered as a land-mark by which he may avoid some superfluous trouble; and be induced, if he thinks proper, to take some of the most prominent of mine as points whence to set forward on a fresh career. I have not sufficient vanity to believe mine of any great consequence; but as his pursuits in the same path appear so nearly allied to those which so greatly engaged my attention, I trust he will excuse my officiousness for thus eagerly endeavouring to arrest his further progress till he has condescended to give them a serious perusal. If the greatest assiduity and accuracy may entitle them to notice, I feel conscious no pains were spared in these particulars; I only lament the occurrence of those untoward events which induced me to relinquish their further progress, and I shall experience no small gratification if they shall ultimately be found of sufficient consequence to facilitate and shorten the labours of others wishing to analyse and throw light upon similar subjects.

A friend of mine has lately presented me with two specimens of calcareous matter, taken from the bladders of two of his horses after they had died from disease,—one weighing nearly ten pounds, in an irregular form,—the other weighing about ten pounds and a quarter, of a conic form. As soon as I can possibly find time for their minute examination, it is my intention to send you the particulars.

I am, sir,

Your most obedient servant,

Ipswich, Aug. 6, 1817. J. Acton.

XIX. *On the Geology of Northumberland.* *By* N. J. Winch, *Esq.*

To Mr. Tilloch.

Sir,—In a memoir on the geology of Northumberland, Durham, &c. published in the fourth volume of the Annals of Philosophy, Dr. Thomson makes the following observation:—"In the preceding rapid sketch I have taken no notice of small patches of the *newest flœtz-trap* which occur towards the north-

east

east parts of Northumberland. I examined several of these places about four years ago, and found them to consist of greenstone rocks *seemingly deposited above the independent coal formation.* This is the case with the rock on which the castle stands in Holy Island. The basis of this island is limestone. The same thing occurs at Bamborough Castle, and in several hills in the neighbourhood of Belford. These facts may have some interest to the geologist, though I did not consider them of sufficient importance to interrupt the general view of the structure of these counties which I have now given."

Though it may appear presumptuous to differ from so able a geologist; yet I am led to think that had Dr. Thomson investigated the rocks at Dunstanborough Castle, at Gunwarden near Barwesford on North Tyne, but especially at Wratchiff Crag near Alnwick, the stratification exhibited at these places would have induced him to draw a different conclusion; for there he would have seen the basalt alternate with the rocks of which the whole district is composed.

At Dunstanborough the cliffs consist of

1. Columnar basalt .. .. 8 to 10 feet.
2. Sandstone .. .. .. 2 feet.
3. Shale (slate clay) .. .. 6 feet.
4. Basalt to below the water's edge.

At Gunwarden—strata of dark-blueish-gray crystalline limestone, from 3 to 4 feet thick, alternate twice with compact basalt. This limestone contains a considerable portion of iron; and in colour, lustre, and the shape of its fragments, so nearly resembles basalt as to render it liable to be mistaken for that substance. To the lime-burner it is of no use. In the neighbouring rivulet casts of the *Madrepora flexuosa*, mineralized by flinty slate, or more properly indurated slate clay, have been detected.

But Wratchiff Crag having been quarried of late years to a considerable extent, and the different beds of which the hill is constructed laid open to view, strengthens the opinion that no flœtz-trap formation exists in Northumberland. The following section, accompanied by specimens, was communicated to me by a friend whose accuracy my be depended upon.

1. Compact basalt, imperfectly columnar .. 20 feet.
2. Indurated slate clay, resembling porcelain jasper 3 feet.
3. Encrinal limestone (containing also bivalve shells) of a dark-brownish gray colour, glimmering lustre, and splintery fracture .. .. } 8 feet.
4. Slaty marl .. .. 4 inches.
5. Crystalline limetone of a light blueish-gray colour, glistening lustre, and fine granular texture .. .. } 3 feet.

6. Slaty

6. Slaty marl .. .. 4 inches.
7. Dark blueish-gray limestone, resembling the Gunwarden limestone .. .. 3 feet.
8. Disintegrated basalt with calcareous spar .. 1 foot.
9. Compact basalt .. .. 4 feet.
10. Slaty marl—lowest.

Dip south-east at an angle of 8 degrees.

Before closing this letter, it will not be amiss to notice a few phænomena usually accompanying basalt in this part of the kingdom, which may in some measure serve to develop its origin. Limestone is often rendered highly crystalline and unfit for lime, when in the vicinity of this rock, as is the case of No. 5 and No. 7, but not No. 3 of the foregoing section. Slate clay is turned into a substance like flinty slate or porcelain jasper, No. 2; and coal is invariably charred when in contact with it. When basalt occurs in beds, its thickness varies much more than that of the rocks between which it is interposed, forming wedge-shaped masses rather than regular strata; and the sandstone on which it reposes is changed for some depth to a brick-red colour: pieces of this description of soft sandstone, taken from below the basalt at Bamborough Castle, broke into spherical fragments on being immersed in water.

I remain, sir,

Your most obedient humble servant,

Newcastle-upon-Tyne, July 20, 1817. N. J. Winch.

XX. *On the Advantages that may be expected to result, from the Study of the Principles of Stratification; with Remarks on the proper Objects of Inquiry in this important Branch of Geology. By Mr.* Thomas Tredgold.

"Men have sought to make a World from their own conceptions, and to draw from their own minds all the materials which they employed; but if, instead of doing so, they had consulted experience and observation, they would have had facts, and not opinions, to reason about, and might have ultimately arrived at the knowledge of the laws which govern the material world."—*Bacon.*

To Mr. Tilloch.

Sir,—In consequence of the discovery of several facts which tend to elucidate the principles of stratification, the science of Geology has acquired an additional degree of interest and importance. Geologists have in a great measure abandoned their wild and fanciful speculations;—they have begun to make observations, and to register facts respecting the present state of the surface of the earth,—and instead of inventing hypothetical solutions

solutions of the most apparent phænomena of its formation, they now attempt to give an accurate description of its structure. Such materials, at some future period, will supply a mind like that of Newton, with the means of establishing a correct theory; for the present state of the earth's surface, is certainly not sufficiently well known, to admit of a satisfactory explanation of its origin.

The knowledge of the relative position of the Strata which form the external crust of the earth, is one of the most important branches of this inquiry; but to render it more useful, there are other objects which should always be attended to in such researches.

It has been observed, that a stratum does not always consist of the same mineral substance throughout its whole extent,—or at least that it often presents the same mineral elements in very different combinations and states; therefore, in a complete description of each stratum, all its principal variations of position, of thickness, of extent and situation of exposed surface, and of mineral character should be accurately described. The petrifactions and shells it contains should be ascertained; and of those that are peculiar to it, correct descriptions should be given;—the uses to which its minerals are applied should be noticed, and the probability of obtaining them in other situations, pointed out;—the nature and qualities of the soil on its exposed surface should be described, and the best means of ameliorating or improving it, suggested. The uses of such information—to the owner of landed property—to the miner—the agriculturist—the engineer—the architect—the manufacturer; and, indeed, to every branch of civilized society, are too self-evident to need detail, and of too multifarious a nature to admit of it here. They only require to be known, to be fully appreciated.

In this as in other descriptive branches of natural history, a concise mode of expressing the leading characters of each stratum, will be necessary, by which they may be described with brevity, accuracy, and precision; as by that means the labour of comparing the facts of different observers will be materially abridged, as well as that of describing them. To accomplish this, it may be necessary to introduce some appropriate terms—for all those which refer to hypothetical notions respecting the mode of formation, should be carefully avoided;—the use of hypothesis is unquestionable, but its very nature renders its language unfit for descriptive purposes. Hypothesis may guide us in our inquiries, and give a tenfold degree of interest to our researches; but still it must rather be considered the instrument, than the end of our labours. To a candid inquirer after truth, the danger of clothing his descriptions of natural phænomena

in

in the language of hypothesis, must be very evident; and the more so, when he considers the narrow views on which hypotheses must be formed, in the present state of geological science.

It may be difficult to form a regular and general principle of classification, independent of some hypothesis respecting the formation of the strata;—a difficulty perhaps to be removed, only by more complete information respecting the stratification of other parts of the globe: however, as far as relates to this island, the strata might be arranged, according to the order in which they follow one another, beginning at the highest in the series. No doubt mistakes will sometimes occur, in assigning each stratum its proper place in the series, but in the progress of the science, these will be corrected.

The attention of geologists is earnestly called to this, or to some superior arrangement of the British strata; for were such an arrangement once made, and a proper and scientific method of describing the phænomena adopted—the number of observers would soon increase, and the knowledge of this important branch of geology would make rapid advances towards perfection.

The landed proprietor will soon find it as much his interest, to know the nature of the strata that form his estate, as to know the number of acres it contains, and a correct mineral survey of his property, will form an useful and valuable appendage to the plan of his estate. And in thus ascertaining the value of his own property, he will have an opportunity of forwarding the progress of science, by adding the result of his inquiries to the common stock;—every mine that he opens, ev[illegible]at he sinks, will either add additional facts or confirm those already known—even in digging a well, something worthy of note may be observed. And should he previously have made himself acquainted with the principles of stratification, he would then have the pleasure of anticipating the general results, while the progress of the work would enable him to ascertain the accidental variations which frequently occur.

But if the study of stratification afford pleasure and useful information to the settled individual; how much more must it afford to the well-informed traveller!—He will no longer need to confine himself to hasty notices of those geological subjects only, that are apparent to the most careless observer—a wider field will open before him, and the structure and mineral production of the country will form one of the most interesting objects of his research. Other travellers have noticed such mineral productions only, as were in use, or plentifully scattered over the face of the countries they have passed through; but the traveller who knows the nature and principles of stratification will be able, not only to give more satisfactory information respecting the minerals already

already known, but also to display the apparently hidden resources of other countries, and to furnish those data, which the extended views of modern science have rendered necessary.

As the labour of gaining any new source of knowledge never fails to bring with it its own reward, by a proportional increase of the sources of pleasure, I hope an attempt to bring that of the principles of stratification into more general notice, may not be without effect. It is a branch of knowledge, which, on account of its useful nature, is perhaps better calculated to become popular, than any other. In proof of the truth of this remark it is only necessary to say, that it includes the principles of the important art of draining land;—that from it the probability of obtaining certain minerals in certain situations may be inferred from the nature of the superior strata, without the expensive process of boring;—that it is calculated to check the delusive mining projects, which have ruined thousands, and at the same time to encourage those which are likely to be attended with success; that it also points out the best methods of working new mines, as well as the most effectual means of extending old ones, with security and profit. I am, sir, yours, &c. &c.

London, August 11, 1817. THOMAS TREDGOLD.

P.S. As the recommendation of any particular branch of science may seem imperfect, without saying something on the means of obtaining it, I have subjoined the following list of works on the subject of stratification. Perhaps some of your correspondents may think pro[illegible]nd it, with critical notices on the comparative merits of the [illegible]ers.

Mr. Wm. Smith's Mineralogical Map of England and Wales: and several numbers of the works he is now publishing, to explain it.

Mr. Farey's Derbyshire Report.

Mr. Bakewell's Introduction to Geology, 2d edition.

The articles "Coal" and "Stratification" in Dr. Rees's New Cyclopædia.

Mr. Sowerby's Mineral Conchology.

Williams's Natural History of the Mineral Kingdom.—And several valuable facts are collected, in

Mr. Whitehurst's Inquiry into the Original State and Formation of the Earth.

Mr. W. Forster's Treatise on a Section of the Strata. Newcastle. 1809.

The Transactions of the Geological Society.

The 25th and following volumes of the Philosophical Magazine, &c. &c. and The Monthly Magazine. T. T.

XXI. *On*

XXI. *On the Work entitled "Chromatics;" or, An Essay on the Analogy and Harmony of Colours.* By Mr. T. Hargreaves.

To Mr. Tilloch.

Sir, — In your last Number you mentioned the publication of "An Essay on the Analogy and Harmony of Colours." On turning over a copy of it, which I have now by me, I remark, that not only are the coloured diagrams incorrect, but that they are at variance with the observations which accompany them. In pointing out this incorrectness, it is not my intention to depreciate the work, but to give the author an opportunity in a future edition of rendering it more perfect, should he consider my remarks deserving of his notice. My objection lies against that part of his work in which he treats on the particular relations of colours. His first example of the white, black, and gray is correct; and in the second, I merely object to the colours employed for showing the three primaries, as not precisely giving the tones required:—the ultramarine inclines rather to purple; the Indian yellow to orange; and the red in its darker shade to orange. But perhaps these colours have been adopted for their durability.

My principal objection is against the compound or derivative tints, given under the denominations of secondaries and tertiaries. But before I proceed, it may be proper to transcribe two or three passages from the work, which are in themselves perhaps unobjectionable, but with which the examples given are at variance.

In section 8 he says: "By the union of these three positive colours (red, yellow and blue) in due subordination, they are neutralized," &c. In section 24 "*Perfect neutrality depends, however, upon a due subordination of the primary colours* in which *blue predominates* in proportion to the depth of the compound, and *yellow is subordinate to red,*" &c. Again, in section 21, "As the *neutralization* or *negation of colours* depends upon the reunion of the three primaries, *it is evident that each of the primary colours is neutralized by that secondary which is composed of the two other primaries, alternately; thus, blue is neutralized or extinguished by orange, red by green, and yellow by purple.*"

Considering all this as correct, and examining the coloured examples of the secondaries and tertiaries, with reference to these principles, they will be found to vary considerably.

In the first place, the secondary or intermediate colour of purple, ought to be such a combination of red and blue, in which the blue should predominate, as when combined with yellow should become completely neutralized. But on looking at the

Example,

Example No. 3, we find it besides being very inferior in brilliancy to either the red or the blue, considerably inclined to the red; so that it seems composed of red and a little blue, and rather neutralized by yellow or black. By adding yellow, therefore, it would not become completely neutralized, but incline to one or other of the tertiaries.

The orange likewise should be a compound of red and yellow, in which the red should predominate; but in Example 4, it is found (assuredly of the colour usually called orange), considerably too much inclined to the yellow; so that the third primary blue, instead of neutralizing, would convert it into an olive or broken green.

The other secondary colour, green, in Example 5, is nearly correct, except that it is rather inferior in brilliancy to either the blue or the yellow.

I come now to the tertiaries, by which are meant a combination of two secondaries, so as to produce a colour in which all the three primaries are united. By this combination it is evident that an extensive variety of tones may be produced, according to the different proportions of the two secondaries employed. But the author means to select such an union of two secondaries, as shall produce an exact broken colour of that primary which enters into the combination of both the secondaries. Thus the tertiary produced by purple and green is required to be of a broken or partly neutralized blue; which will of course, as implied in Section 22, be completely neutralized by orange. But on referring to Example 6, we find that the author has produced an olive, a colour in which greenish yellow predominates; which might be expected from the incorrectness in the tone of the purple not allowing a sufficient quantity of it to be used for giving the tint required. This olive instead of being neutralized by the orange would change into broken yellow. Here of course the error is not confined to the coloured example, but the name adopted shows the author himself to be in error. The other two tertiaries are likewise incorrect,—the citrine being rather too much inclined to orange; and the russet is more a broken orange, than the partly neutralized red which it ought to be. The remaining examples are, of course, infected with the same errors, as being composed of the tints and colours already described.

I do not at present enter into any consideration of the author's ideas on the harmony of colours, as I have not yet found leisure to understand and consider them. But as I was struck with his adoption of the double triangle for the purpose of illustration, and the general agreement of his ideas on colours with my own, as inserted in your Number for March 1817, I was

 tempted

tempted to examine that part of his work; the result of which examination I here send you; and if you consider it, as I do, of some importance to art, I doubt not of your admitting it to a place in your valuable publication.

I am, sir, yours, &c.

Liverpool, Aug. 16, 1817. THOMAS HARGREAVES.

XXII. *Notices respecting New Books.*

An Inquiry into the progressive Colonization of the Earth, and the Origin of Nations; illustrated by a Map of the Geography of Ecclesiastical and Ancient Civil History. By T. HEMING, *of Magdalen Hall, Oxon.*

[Concluded from p. 72.]

"THE progress which we have made already assures us that there are mountains so situated as Moses hath pointed out to us—that these mountains appear to join the popular Ararat of Armenia, or the Gordian mountains—that there are traditionary reports of the ark having lodged upon the mountains of Ariana, which are a part of the same tract—that there are, about those parts, names which appear to have generated from Ararat, Gordus, and Cardu—all which considerations, though they have a tendency to confirm the declaration of Moses, would be very much too flimsy and insufficient, without some additional strength.

"There are some evidences of the early population of these parts, which may be mentioned as correlative arguments in favour of the general question. Megasthenes relates, that the old inhabitants of India were divided into 122 nations, all originally descended from the sons of Noah, before their journey to the valley of Shinar.

"Nearly 2000 years before the Christian era, Semiramis invaded these eastern settlements with an army of above 4,000,000, if Ctesias and Diodorus do not exaggerate (though we can hardly suppose they do not). Staurobates, the Indian general who we are told met this enormous force, had, they say, an army equally numerous, and obtained a complete victory over Semiramis, who was slain in the fight. Deduct whatever may be necessary to reduce these armies to credible numbers, and then the population of each of these adjacent countries must have been, beyond a doubt, exceedingly great—probably, and almost certainly, greater, at this early period, than any other contemporary nations of the whole earth.

"It is probable, that had Armenia been the point of disembarkation, the adventurers would have reached Shinar in much less

less than a century; as the routes along the Tigris and Euphrates are so accessible and easy: and on the other hand, the distance from the east of Persia, and the time of arrival at Shinar, seem to be much more proportionate than those of Shinar and Armenia.

"It is improbable that the fertile plain of Jordan would have been destitute of proprietors for so long a time as 450 years after the flood, if the ark had settled so contiguously as the Gordian mountains; whence, the descent to Jordan would have been so facile and convenient; and we find (Gen. xiii. 11) that the whole of this fine country was open to the choice of Lot, who took possession of it without opposition: and here may the rapid progress of population be particularly instanced; for in a few centuries afterwards, this became the most populous district that the earth ever contained.

"It is probable, that if Armenia had been the focal point, Europe would have been colonized before India; but it is agreed beyond dispute, that India was planted much earlier than Europe; and it is moreover certain, that the most eastern parts of Europe were peopled before the western; which, had the migration been from Armenia, would not have been the case.

"It is probable, had the first post-diluvian progress been made from Armenia, that Syria and Asia Minor would have become famous settlements before Egypt; because, from their contiguity, they could not fail of being soon discovered; and their inviting aspects, both with regard to climate and fertility, would certainly have insured the sojournment of whatever colonies chanced to come towards them—but it is certain that Egypt was overspread with inhabitants long before Syria or Asia Minor; and it is therefore probable, that the first Egyptian colony proceeded coastwise from the Indus; whereas, had it passed from Armenia, it would most likely have gone through some part of Syria, and would, of course, have occupied it in the way to Egypt; which was not the case.

"Although neither the perilous arduities of mountains, nor the terrible menaces of oceans, were insuperable and daunting to the daring enterprisers; yet the even valleys and less rugged tracks of rivers were most readily pervaded: and if we search the surface of the whole globe, no spot seems to distribute so many streams as that part of ancient Asia, whence they issue on both sides of the mountains from Herat to Gaur, and run in all directions towards the north and towards the south.

"This sublime tract of heights, though in themselves steadfast and durable as time itself, have, as every latter circumstance rolled on and involved its forerunner, fluctuated in name with the successive changes. By Megasthenes, Strabo, and Pliny, they are called Paropamisus, from the ancient Persian province of

that name. By Cluverius and Mela they are termed Taurus, from their being supposed to be a continuation of that mountain —by Aristotle and Quintus Curtius they are called Caucasus—by Pliny, Cellarius, and Ptolemy, they are mentioned by the name of the Hyrcanii Montes, from their passing through the country of Hyrcania—by Arrian they are designated as Mons Matieni—by others they are since called Himmaleh and Hindoo Koh: but we know that none of these is a genuine name; indeed, they are partial only, and such as have accidentally been applied to them; as we learn from many of the Greek authors. But are we not to suppose that these important mountains, before any Grecian had existence, were denoted by some name? And is it not probable that they were known to the predecessors of Moses by the general title of Ararat? Or, may we not justify the presumption, that Moses, from an intimate knowledge of their character and consequence, endued them with the appellation of the Mountains of Ararat?

"Hareius denominates the whole range from the Euphrates to the Ganges 'the Montes Araratis.'

"Dr. Heylin condemns the opinion of the ark having rested in Armenia, and supposes it more likely to have remained on some part of the Imaus mountains in India, which are somewhat further north-eastward from the spot which we propose to consider as the place of disembarkation.

"Dr. Stukely, who has investigated the subject with the sagacity of a philosopher and the discrimination of a critic, concludes the seat of the ark, after the flood, to have been rather westward of the head of the Indus, and about the point of each longitude to which the map of scriptural and classical geography extends.

"We might add numerous other conjectures of the same kind; but the testimony of one commentator who has patience to sift, judgement to discern, and impartiality to decide, is of more weight and value than a cordon of those who copy one another's errors: and as the purpose of this debate will require but few more corroborations and arguments, we shall, after advancing one or two others which possess, in our opinion, the most consequence, bring the question to an issue.

"If we search to discover them, there may generally be discerned some extraordinary signs of divine omniscience and contrivance in every act of the Almighty Master; and it is no less than marvellous, that the grand streams of the Indus, Oxus, Jaxartes, with some branches of the Ganges, and a great many other rivers, derive their sources from about the central district of the three principal divisions of the earth, and which is in that part of ancient Aria, or Ariana, where we propose to consider

that

that the remnant of the wreck of human nature first released themselves from the fabric which had saved them from the universal catastrophe. This situation perfectly accords with the point to which Moses has referred us; and seems to have otherwise more probabilities in its favour than any other position which we have seen laid down.

"It is not here intended to be insisted, that probability ought to be received as proof: but problems of history so intricate and inexplicable as the present, cannot be solved according to the principle of mathematical demonstration: proceeding then from probability to probability is the only way of getting towards the fact; and where numerous probabilities corroborate and support one another, they are, or ought to be, esteemed almost tantamount to physical truth.

"It must be recollected, that the principal object to be established from the present inquiry is, that some position, consistent with the express asseveration of Moses, be considered as the resting-place of the ark: and that the point to be assigned must have a much greater eastern longitude than any part of Armenia; otherwise it will be contradictory rather than conformable to what Moses has so unconditionally and unequivocally declared.

"That part of the ancient Persian province of Aria, extending from modern Herat, or Harat, to the country of the Gaurs, or according to the orthography of some, the Giaours, along the tract of heights called Hindoo Koh, is the place to which the investigation seems to lead, as having, according to numerous probabilities and circumstances, most likely been the receptacle of the ark, after the secession of the waters from the face of the earth:—but before we entreat the suffrage of our readers to this opinion, we will abstract and arrange, in a brief form, some of the chiefest motives which have contributed to the preference.

"First—Moses declared in perspicuous terms, that 'the ark rested on the Mountains of Ararat,' and that the emigrants 'journeyed *from the east* till they came to the Plains of Shinar'—therefore, finding, as we do, that the mountains of ancient Aria in Persia are, though at a great distance, connected with those of Armenia, and that they are relatively situated with regard to Shinar, as stated in the Scripture; these were motives which, in some degree, influenced the inducement to propose them as the probable place where the ark rested after the flood.

"Secondly—It is not to be imagined that the emigrants proceeded in one direct and uninterrupted progress from Ararat to Shinar; yet may some idea of the relative distance between these places be formed by the portion of time which the journey consumed. Aria is not objectionable on account of its

distance from or contiguity to Shinar, and the migration from one to the other may very readily be supposed to have required as much time as the Scripture signifies—this apparent proportion of the time and distance was another motive that biassed the proposal.

"Thirdly—Some very judicious inquirers on the same subject, are decidedly of opinion that the ark rested somewhere along this tract of mountains towards Tartary or India; and their not having all consented to one spot is no derogation to the main point; for they all propose a site eastward of Shinar, and therefore do not deviate from the Mosaic text. Along this vast ridge, to which all ascribe the memorable event, we, for the foregoing and other reasons, consider Aria to be the most probable point; and as this opinion is not incompatible with that of Hareius, Ortelius, Drs. Stukely and Heylin, Shuckford, Wilson, and other eminent authorities, we have, with greater confidence, been induced to propose it.

"Fourthly—From not having been able to discover any other primitive name of these mountains, it is conceived that Ararat ought not to be considered as a term appropriated to any particular part, but to have been much more extensively applied than has been generally imagined; and from the many names attached to places and things, in the vicinity of Aria, that appear to have some affinity to the word Ararat, additional instances in favour of the proposal have also been deduced.

"Fifthly—This Persian district includes the central point of the three grand divisions of the earth—that is to say, of Europe, Asia, and Africa—which, considered as so regarded in the omniscience of Providence, and thereby suited for promoting in somewise the great scheme, was also additional weight to the reasons for the proposal.

"Sixthly—From its seeming compatible with the incomprehensible wisdom and œconomy of the Supreme to afford facility at the outset to the 'overspreading of the earth,' and as the courses of rivers are most free from impediments, and supply one of the most essential articles of human subsistence, it is natural to suppose that the itinerant corps would take their routes along the tracks of currents; and from the multiplicity of these which are distributed northward and southward from the central acclivities of Aria, in a manner not to be found in any other region of the earth, it was a consideration that powerfully augmented the force of the other motives which induced the proposal.

"Seventhly—Herack, Yerac, or Iraq Agemi, signifying the country of the mountains, is southward of the Caspian Sea, about ancient Hyrcania. No part westward of this can be adopted as the place where the ark rested, because the Scripture objects to it;

it: any where more eastward along the same ridge may, because the Scripture allows it. To say that the ark rested in Armenia is therefore dissonant to the prescript of Moses, unless Armenia in immemorial ages extended to Hyrcania, which is not altogether improbable;—but it is much more likely that Ararat was of this, and even much greater extent, before it became confounded with Armenia; and the identity of these two places, which ought to be distinct, has been very perplexing, deceptive, and injurious.—Tenacious, therefore, of a perfect faith in Moses and his interpreters, we must reject altogether the pretension of the ark having rested northward of Shinar, and adopt the more congruous proposition of the extension of Ararat beyond Aria, because there are many reasons to authorize it, and no substantial objection seemingly to the proposal*.

"Lastly—The tradition mentioned by Wilson, in his 'Asiatic Researches,' of the ark having lodged upon Aryavart may be added, because it is perfectly consonant to Scripture, and because it is of as much consequence as a tradition can possibly be, on account of its derivation from an indigenous source; whereas every tradition relative to Armenia is from the report of aliens, who were unacquainted with the territory for full 1700 years after the event they presume to recount had taken place.

"Having now summed up the main arguments which have been brought forward in this intricate inquiry; and which, whether scriptural, theological, physical, geographical, etymological, testimonial, or traditional, have all one uniform tendency—and are deemed, altogether, sufficiently cogent to authorize the conclusion, that the country of ancient Aria, in the east of Persia, comprehends that part of the mountains of Ararat where the ark rested after the great deluge, when Noah and his three sons, with their wives, were all the remnant that survived to repropagate mankind, we shall therefore hereafter consider ourselves warranted in alluding to this as the focal point whence the whole earth has been overspread with all the varieties that have existed, since the deluge, of the human race."

In the third chapter the author treats "of the dispersion and several settlements of the descendants of Noah, whom we find enumerated in the book of Genesis." The fourth, which concludes the work, is entitled "Considerations on the time of

* "May not Ararat and Aria, also Arachosia, Arasacia, &c. have been named to memoralize settlements of the descendants of Jerah, the son of Joktan (called by the Arabs *Arah* or *Yarah*), as Moses informs us that the Joktanites were stationed from Mesha (signifying a desert) to Sephar, a mount of the East, which Wells places in Persia: and Eustathius, Hieronymus, &c. derive the Bactrians, Hyrcanians, Caramanians, Scythians, &c. from the sons of Joktan.

the general dispersion, and the number of persons that had arisen. —The confounding of language.—Genealogy of the Hebrew and Greek bibles examined.—Original nations founded subsequent to the first dispersion.—The earliest nations of whom there are written documents; and the results and connexions relative to them which may be derived from the foregoing sketches."

Mr. Heming's valuable map, which should have a place in every library, will be found a most useful auxiliary to all students of the Geography of Sacred and Ancient History.

An Essay on the Nature of Heat, Light, and Electricity. By CHARLES CARPENTER BOMPASS, *Barrister at Law.* 8vo, 276 pp.

This is a work of uncommon merit, and will, we are confident, be well received by those whose minds have been properly disciplined by the strict laws of the inductive philosophy. The author in his preface apologizes for offering an essay on a branch of natural philosophy, unsupported by experiments of his own: but we think the chances are at least equal, that he would not have produced a more useful work, had he had experiments of his own to detail, and the results to explain. He has done better in founding his observations on the labours which others, "in return for the honours so justly paid them, have surrendered for public use;"—for, had he offered new experiments and new results, the attention of the reader (if not his own) would have been diverted from the main object of the essay; or at least divided, and the author would have produced less effect.

The work is divided into chapters, and these into sections. *Chap.* I. ON THE NATURE OF HEAT. § 1. On the Materiality of the Cause of Heat. § 2. On Attraction for Caloric, Latent Heat, and Evaporation. § 3. On the Communication of Caloric. § 4. On the Reflection of Caloric. § 5. On the supposed Repulsion between the Particles of Caloric—and the Elasticity of Gases. § 6. On the Nature of the Attraction for Caloric.—*Chap.* II. ON THE NATURE OF LIGHT. § 1. On the Mutual Relations of Light and Caloric. § 2. On the Reflection of Light—and Visibility of Bodies. § 3. On the Component Parts of Light—and the Causes of Colour. *Chap.* III. ON ELECTRICITY. § 1. On the Formation of the different Kinds of Electricity. § 2. On the Nature of Electrical Attraction. § 3. On the Franklinian Hypothesis. § 4. On the Combination of the Two Kinds of Electricity, and the Identity of the Compound Ethereal Fluid with Caloric and Light.

It would not be justice to the author to offer an analysis of a work which is wholly argumentative: we shall therefore confine ourselves chiefly to a statement of some of his conclusions:

"Caloric

"Caloric (for the reasons adduced) is certainly contained in every body but the coldest, and, no one will hesitate to conclude from analogy, in that also. If then there be in all bodies an invisible imponderable fluid or substance, capable of producing all the phænomena of heat, it is surely unphilosophical to seek for any other cause of it, where this exists." The author therefore assumes that this *matter* is the only source of heat. He employs the name commonly used, "caloric," but "without any intention to express an opinion that it is a simple substance."—"If caloric be matter, it possesses inertia; and consequently, when once without motion, unless acted on by some other body, it would remain for ever at rest."—"The only powers by which matter unaided can act upon matter, are attraction and repulsion."—Opposite powers in similar bodies where one is sufficient are rejected in sound philosophy;—but we cheat ourselves with terms. What is "radiation" but another name for "repulsion?"—"The law is universal, that all bodies attract caloric—but the degree in which they attract it is different in different circumstances."—"That which has been called latent heat is only the effect of an increased exertion of the attraction for caloric, produced by the modifications of the attraction of cohesion."

For the reasons stated by the author, "caloric must necessarily be imponderable."—Though the passage of caloric is produced simply by attraction, the phænomena are modified by circumstances. "Motion is given to caloric at its issuing from the heated body. But the attracting power continues to operate on the caloric as it proceeds, and with a force increasing as it approaches the attracting body; consequently the motion is continually and increasingly accelerated. The ray strikes therefore upon the attracting body, with a force greater than the then acting attraction, by the sum of the force of all the attraction preceding. Suppose the body attracting the caloric to be perfectly hard and impenetrable, and the ray would rebound or be reflected; the then exerted attraction which alone would tend to continue it in contact, being so evidently less than the force of the attraction accumulated through its whole progress. Although no body is impenetrable to caloric, it is generally admitted that the particles of every body are so. All the caloric therefore, which should not pass between the particles, but strike immediately on them, would be reflected as though it impinged on a body perfectly hard and impenetrable. Accordingly, it is found by experiment, that a very large portion of caloric is reflected from some bodies to which it is attracted. Metals particularly, attract caloric with considerable force, in proportion to their volume; but when highly polished, a very small part is able to

to penetrate them. Suppose then a plate of polished metal, to attract caloric from a distant object. The particles of caloric will strike upon its surface, but the greater part will be unable to enter it. That part of the ray of caloric therefore, which cannot penetrate, impinging upon the polished metal with a force greater than the attraction at the surface, and its elasticity being perfect, it will rebound or be reflected, with a force equal to the excess; the attraction acting upon it on its return, with a continually decreasing, retarding power. If, however, another body be placed so as to exercise its attraction in the course of the reflected ray, the retarding attraction will be opposed by the force of that attraction, and the course of the returned ray will be continued. If the heated body be placed in the focus of a concave metallic mirror, the caloric would be attracted by it, and almost the whole quantity attracted, would be reflected from its polished surface in parallel lines. The metal, however, does not act with an equally retarding attraction upon the ray of caloric, because a concave mirror will always attract with the greater power, objects in the line of its focus. This will be very evident, if that ray be considered, which impinges upon the extreme circumference of the mirror, when it will be perceived, that on its return, at the same distance from the part on which it struck, it will be much less under the influence of the attraction of the mirror, than in the heated body situated in the focus. This difference of attraction must have a very considerable effect; for a small force will convey a ray of caloric to a great distance, as may be easily imagined from its usual wonderfully rapid motion. And if a small direct force be sufficient, a small excess over a counteracting force is equally effectual. If the retarding attraction therefore, of the first mirror, with all its opposing circumstances, be but in a small degree less than its accelerating attraction, caloric may be conveyed to a considerable distance. But the small force with which the caloric passes from the mirror, beyond the distance of the heated body, being only the excess of the direct over the retarding attraction, does not lessen the quantity, in the same degree, as it would if it arose from a small power of attraction. The quantity passing is that which is attracted by the mirror from the heated body, with the deduction of that quantity which the mirror itself retains, and that which will be retained by the attraction of the atmosphere, and other such circumstances. If a second mirror be then placed opposite to the first at a moderate distance, the rays will impinge upon it, and most of them be reflected to its focus. The second mirror will not only reflect the rays which strike upon it, it will also assist their progress by its own attraction

tion for them; and if its attraction should be great, it would affect that of the first mirror. If, for instance, a piece of ice be placed in the focus of the second mirror, the ice will rapidly absorb the rays of caloric, and attract the caloric from its surface. This will increase the attraction of that mirror, and consequently increase the rapidity, and the force of the whole process. The surface of the first mirror will have its attraction increased, and the temperature of the heated body will more rapidly fall. The effect of a single mirror in reflecting caloric, passing from a body placed opposite to it, at a moderate distance, may be explained upon the same principles. The caloric in that instance would be simply attracted by the mirror, and reflected directly to its focus, and would raise a thermometer placed there, with a power greater than without the mirror, in proportion to the concentration of the rays. The same laws which exist in other cases in the attraction of caloric from body to body, regulate its conveyance in these cases from the heated body to the mirror, between the two mirrors, and from the second mirror to the colder body;—the mirror, only being required to be composed of a substance which attracts caloric, without readily permitting its entrance into it."

The author next examines the supposed repulsion between the particles of caloric, and the elasticity of gaseous bodies. The increase of the volume of bodies by the addition of another substance—even if that substance be caloric—is what ought to take place, and furnishes no proof of the existence of repulsion. Caloric causes not repulsion in gases, but expansion; and they obtain or retain the substance which causes this expansion by attraction. We have no evidence of the existence of a power of repulsion.

All solid bodies when raised to a certain temperature become luminous. Light is communicated with caloric, and in some proportion to it. It must be conveyed either by some affinity which it has for caloric, or all bodies must have an attraction for light, in some proportion to that which they have for caloric. The latter is the more probable hypothesis. Bodies are not luminous by reflected, but by emitted, light; and they emit it in consequence of the attraction of some other body: if emitted directly to the eye, "it must probably be by the attraction of the eye. Nor is it probable that the *quantity* of light required for vision, can bear more than a very minute proportion, to that emitted from a heated body. It is probable therefore, that, whatever relation light may have to caloric when bodies are luminous, the light which enters the eye must do so in the same manner and be governed by the same laws as the caloric. But if light be capable of producing expansion, and be attracted, and contained

tained by all bodies in the same manner and proportions, as caloric, what difference can we state as existing between them, except that motion, by which, whenever possessed by caloric, bodies are rendered visible?"

"Bodies are luminous by light emitted: they are visible by light reflected from them."—"If a red-hot ball be placed in the focus of a concave mirror, both the light and the caloric will be reflected by the mirror, and may be collected by an opposite mirror and again reflected, in the same manner as caloric has been before described."—"Bodies are seen by the light which they themselves attract." In proof of this—"their colour arises from the nature of the light which *impinges* on them. Thus a coloured ray, separated by the prism, or other means, gives its own hue to every object on which it is thrown. If therefore bodies had no influence in regulating the nature of the ray of light which should approach them to be reflected to produce vision, their colour would always depend upon, and vary with, their situation. This, however, is shown by every moment's observation not to be the fact. The experiments last referred to, are directly adverse to the theory suggested by Sir Isaac Newton on the subject. He supposed, that all the rays that fell upon any body might be absorbed, except that part which formed its colour; and the reflection of that part of the ray, rather than any other, he suggested, might arise from the difference in magnitude of the particles of light. If this hypothesis were just, a separated coloured ray ought to be wholly absorbed by every body not of the same colour, instead of giving them a tinge unnatural to them. Upon that theory too, no part of a ray of light could be reflected more than once between bodies of different colours; and every kind of body, except that which absorbed the smallest particles, would reflect, on account of their magnitude, smaller particles than those which it received. The division of the solar ray, however, by that truly wonderful man, has been the chief discovery yet made respecting the nature of light, and the most probable guide to all others which may be made in future."

In the third section of Chap. II. the aim of the author is to show that light is a compound ethereal fluid composed of only two simple fluids, combined in different proportions according to circumstances; and that caloric is another modification of the same compound ethereal substance. We cannot convey an adequate idea of the arguments by which this is enforced, but by inserting it entire, which we shall do as soon as we can make room for it.

On Electricity, the conclusion of the author is, that the two kinds of electricity which are known by the names of negative and positive are, in their combined state, identically the same fluid

fluid with caloric and light: but for the reasoning by which he supports this opinion we must refer our readers to the work itself.

Under the title "CONCLUSION," the author proceeds to consider the "effects of magnetism, which bear so near a resemblance to those of electricity, as to leave little doubt that the causes must be very similar." The explanations offered, assume that there are two ethereal fluids—as in electricity—inferred from "incontrovertible experiment."

The following works have just been published:

The Principles and Application of Imaginary Quantities; to which is added some Observations on Porisms, being the first of a Series of original Tracts on various Parts of the Mathematics. By Benjamin Gompertz, esq.

An unlimited Daily Calendar, serving for every year, before and after the Christian æra, both for the old and new styles. By J. Garnett.

A Treatise, containing the results of numerous experiments on the preservation of timber from premature decay, and on the prevention of the progress of rottenness, when already commenced in ships and buildings, and their protection from the ravages of the *termite*, or white ant; with remarks on the means of preserving wooden jetties and bridges from destruction by worms. By William Chapman, M.R.I.A. Civil Engineer, &c.

Mr. Thomas Forster has just published a Sixth Edition of his Observations on the Natural History and Brumal Retreat of the Swallow; illustrated by fine engravings on wood, by Willis, and interspersed with Anecdotes. To which is added Extracts from a Journal of Natural History, and a Catalogue of Birds which are found in the Island of Great Britain.

He has likewise published Observations on the Casual and Periodical Influence of Atmospherical Causes on the Human Health, and Diseases, particularly Insanity; with a Table of Reference to Authors who have written on Epidemical and Periodical Diseases. This work is illustrated by some novel cases, and the author endeavours to place the periods of insanity and other disorders of the brain and nervous system in the most important point of view, from the necessity of beginning the curative proceeding at particular stages of the disorder. He classes the atmospherical influence into two sorts: 1. That which appears casual or happens at uncertain periods, exciting epidemics and other atmospherical complaints. 2. That which has observable periods:—this he subdivides into annual, monthly, and daily periods. He notices also certain other periods which belong exclusively to particular diseases. The tract is interspersed with

with anecdotes, and concludes with some observations on Suicide, which place that crime in a new point of view, considered as frequently resulting from a slow and often unperceived sort of insanity.

Mr. William Phillips, of Tottenham, has published a small work on Astronomy for those unacquainted with the Mathematics. He therein mentions that a work on Meteorology is forthcoming from the pen of Mr. Luke Howard.

Several more works from Dr. Spurzheim are expected from Paris in the course of a short time to be published in England.

Decorative Printing.—It is now some time since Mr. William Savage issued Proposals for publishing Practical Hints on Decorative Printing, illustrated with fac-similes of drawings printed in colours by the type-press. The preparations for this singular and unique publication are, we are happy to say, in a state of great forwardness. We have seen some of the embellishments, imitations of water-colour drawings, so close as not to be distinguished from real drawings. They are produced by the application of various tints by means of a succession of blocks, so managed as to produce all the gradations of light and shade, without the least harshness or confusion. By this means the finest drawings may be multiplied to an inconceivable extent—a desideratum which promises to be of the greatest advantage to science, especially in all the different departments of natural history; putting it within the power of a traveller, at a comparatively small expense, to lay before his readers correct representations of the various objects with which it may be desirable to illustrate his work. But these, though important objects, are in one sense but secondary in Mr. Savage's work. He not only shows by his own specimens that all this is practicable; but he gives the necessary instructions to enable others not only to execute and apply the different blocks, but to prepare all the various inks and tints necessary for these and for every species of letter-press printing. On this point the instruction to be communicated is most important, as he has brought to perfection the art of making printing-inks without the least particle of oil entering into their composition, or any thing that can sink into the paper, or spread from the impression and discolour the paper. This is an object of the greatest value; for numerous publications, on which every degree of attention has been bestowed by the printer, are often rendered of comparatively little value, in a short time, by the discoloration of the paper occasioned by the spreading of the oil. We are sorry to observe that Mr. Savage has limited his impression to what we consider as too small a number for a work of so much value, 100 large (imperial 4to), and 250 small (demy 4to): for, as the blocks are all to be destroyed

stroyed as soon as the work is printed, many who may wish afterwards to possess a copy, and to whom it might prove highly serviceable, must be disappointed.

New Variation Chart.—All the variation charts hitherto published, have been only transcripts of Dr. Halley's original chart, with few corrections for the change of variation since his time, and none of them extending beyond the Atlantic and Indian Oceans. Navigators have therefore long regretted the want of an accurate variation chart, comprehending the whole circuit of the navigable ocean and seas of our globe. To supply this want, Mr. Thomas Yeates has, with much labour and care, constructed a variation chart of all the navigable oceans and seas between latitude 60° north and south, from accurate documents obtained of Spanish surveys in the Pacific Ocean; journals at the Hydrographical Office, Admiralty; and at the East India House; collated with tables of the variation recently formed from the observations of different navigators. This chart is delineated on a new plan, all the magnetic meridians being drawn upon it throughout, for every change of one degree in the variation; and it will be elucidated with explanatory notes, and a brief statement of the late discovery of an aberration in the variation, resulting from the deviation or change of a ship's head from the magnetic meridian, accompanied by the rules invented by the late Captain Flinders for correcting the same. It is to be published by subscription (price half-a-guinea) at Messrs. Black, Parbury, and Allen's, No. 7, Leadenhall-street; Mr. E. Troughton's, No. 136, Fleet-street; and Mr. Bates's, Poultry.

XXIII. *Intelligence and Miscellaneous Articles.*

To Mr. Tilloch.

Sir,—I beg to add something explanatory relative to an expression of mine in the communication you were pleased to insert in your last Number. I said that the safety promised by the 'Davy' was questioned by those "who ought to have known better." Certainly there never was any thing more unphilosophical than the opposition it met with from them, and this even continued after it had been *proved secure* in the mine itself. Some raised their voice against it who never saw it, and had only heard of it through the medium of imperfect description. Others had seen it, but such had never made the experiment, and they yet rudely questioned its efficiency. Others still more daring, subjected it to experiments totally unconnected with the phænomena of the mine, and, determined to pervert its value, gave a false

a false estimate of its merits. Such is literally a portraiture of the character of the opposition made to the introduction of the safety-lamp. I submit it to the liberal and enlightened mind, whether it would not have been more philosophical to have *first proved* whether this instrument, introduced with such important recommendations, was really so wondrously endowed, and then to have given their opinion on its value or demerits?

Having paid considerable attention to the action of vegetable and animal poisons on the system,—the article which appeared in your penultimate Number, On the Poison of the Viper, could not fail to interest me. I have long believed that *animal* poisons could be received into the system without injury, and that to produce their proper effect they must be introduced into *the circulation*. The conclusions of the paper in question are beautifully corroborated by the following extract from a letter to me, by Mr. Campbell, the African traveller: "The Hottentots believe, that if they *swallow* the serpent's bag of poison, a sting or bite from a serpent will do them no harm. Several of my Hottentots assured me they had done it;—one, who asserted it, was a *Christian*, who I think would have sooner submitted to have been torn to pieces by a tiger, than to have uttered a *deliberate* lie: so I fully credit it."

The article in Dr. Thomson's Annals for last month, On the Test for corrosive Sublimate, &c. calls to my mind a very excellent and delicate one for the detection of mercurial salts.—Rub a little corrosive salt or calomel on a piece of silver, or suffer a drop of a solution of muriate of mercury to rest upon it; a stain of a *coppery* colour will be left, and this, after a very high degree of dilution.

If I might be permitted to remark on Mr. Tatum's Experiments on Vegetation, I would say that they are liable to as many and as great objections as any other that I have seen detailed. They were subjected to a *confined* instead of a free atmosphere, and to *mercurial* effluvia—the temperature of the included medium was unnatural, and they would be excluded from those thousand sources of vicissitude which constitute the spring of all their beauty. I shall still hold unchanged the opinion I have long maintained as the result of direct experiment; namely, that the quantity of carbonic acid evolved by plants will bear but a pitiful proportion to the floods of oxygen poured out upon the atmosphere by the exercise of the vegetative functions—My mind therefore rests contented on the experiments of Priestley and Ingenhousz since corroborated, in contradiction to those of Ellis and Tatum. These observations will receive additional weight from the following deductions. It is notorious that oxygen is evolved from plants *during the stimulus of light*, and that vegetation

vegetation will continue some time healthy in an atmosphere of *carbonic acid gas*. That the vegetable functions act differently from those of animals, is evident from the fact, that until the oxides of iron are *healthful* to the animal œconomy, they are *destructive* to the process of vegetation. If the carbonic acid gas was at all equivalent to the oxygen set free, *whence comes the carbon* which builds up the curious structure of the plant? The partial quantity of carbonic acid which plants respire, is evolved during *night*; and this being condensed by the cool of the evening, and mingling with the dews of this season of repose, will not deteriorate the atmosphere, but be absorbed by the soil on which it falls, and minister anew to the requirements of the plant. Besides, in winter, the plant being denuded of its foliage has its inspiratory and expiratory organs comparatively suspended; while the period when these powers are uncontrolled and most active is marked by a much longer sojourn of the sun above the verge of the horizon. And I may add in conclusion, the sentiment of Brisseau Mirbel: "In Europe, while our vegetables, stripped by the severity of the season of their foliage, no longer yield the air contributing to life, the salutary gas is borne to us by trade-winds from the southernmost regions of America. Winds from all quarters of the globe intermingle thus the various strata of the atmosphere, and keep its constitution uniform in all seasons and in all elevations."

Being in the habit of frequently experimenting with the blow-pipe of condensed oxygen and hydrogen, Dr. Clarke's late communication in Dr. Thomson's Annals of Philosophy afforded me particular interest. Besides oil from its tranquil ebullition affording no *index of safety*, the disadvantage pointed out is as [illegible]ected as important. During my course of chemical lec[illegible] at Greenock, I used *water* in the safety cistern, and my experiments were splendid and imposing. In the use of this instrument at Paisley, in my late lecture, the illustrations were feeble in effect and unimposing, and I have often since wondered at the circumstance;—now at this time I used *oil* instead of water, and Dr. Clarke has fortunately solved the question. It appears then that *oil* will never do.

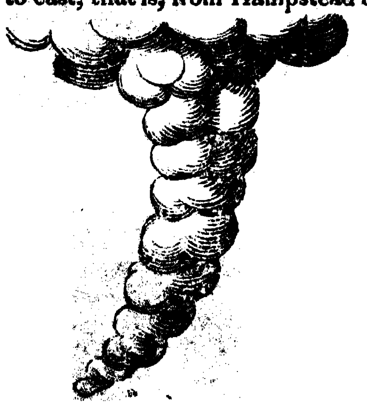
I may conclude these *miscellanea* by adverting to a very curious circumstance which occurred to me here during my lecture on galvanism. I used three *porcelain* troughs with *triads* upon the principle of Dr. Wollaston. The fluid medium employed was diluted nitric and muriatic acids. I had omitted inadvertently the beautiful experiment of the ignition of platinum wire, until the action of the troughs was so feeble that it would not affect *a hair's breadth* of the metal. I immediately proposed, by *way of experiment*, to withdraw the plates from the cells,

cells, and try the effect of a few *minutes' exposure to the atmosphere:*—the effect was singular and interesting: for when the plates were returned, *upwards of six inches* of the platinum wire were instantly exalted to a WHITE HEAT. This important result will immediately bring to your recollection some analogous experiments of Mr. Parrett, jun.; and it follows that by the application of mechanism to raise and lower the plates*, we can *at pleasure* renew if not *increase* the action, *without additional acid.* I have frequently repeated this since, and always obtained an increased action. Your obedient humble servant,

Whitehaven, Aug. 18, 1817. J. MURRAY.

WATER-SPOUT.

It happened to the editor of the Monthly Magazine, on the 27th of June, about seven in the evening, to witness the formation, operation, and extinction of what is called a water-spout; a phænomenon which in all ages has puzzled philosophers and encouraged the superstition of seamen and the vulgar. He was in the house north of the chapel at Kentish Town, and his attention was drawn to a sudden hurricane which nearly tore up the shrubs and vegetables in the western gardens, and filled the air with leaves and small collections of the recently cut grass. Very dark clouds had collected over the adjoining country, and some stormy rain accompanied by several strokes of lightning followed this hurricane of wind. The violence lasted a few minutes; and the writer being drawn to the eastern balcony, it was evident that a whilwind agitated the variety of substances which had been raised into the air. The storm proceeded from west to east, that is, from Hampstead over Kentish Town towards Holloway. In about five minutes, in the direction of the latter place, a magnificent projection was visible from the clouds, like that on the margin:



It descended two-thirds of the distance from the clouds towards the earth, and evidently consisted of parts of clouds descending in a vortex, violently agitated like smoke from the chimney of a furnace recently supplied with fuel. It then shortened, and appeared to be drawn up towards the stratum of clouds; and presently it assumed the following appearance:

* As in Mr. Pepys's Apparatus.—ED.

It finally drew itself into the cloud; but a small cone, or projecting thread, of varying size and length, continued for ten minutes. At the time, and for half an hour after, a severe storm of rain was visibly falling from the mass of clouds connected with it, the extent being exactly defined by the breadth of Holloway, Highgate, and Hornsey. About two hours after, on walking from Kentish Town towards Holloway, it was found that one of the heaviest torrents of rain remembered by the inhabitants had fallen around the foot of Highgate-hill; and some persons having seen the projecting cloud, an absolute belief existed that a water-spout had burst at the crossing of the new and old roads. On proceeding towards London, various accounts, agreeing with the superstition or preconceived notions of the bye-standers, were given; but in the farm-yard at the three-mile stone it appeared that some haymakers were stacking some hay from a waggon which stood between two ricks, that the same whirlwind which passed over Kentish Town had passed over the loaded waggon with an impetus sufficient to carry it above twenty yards from its station, and to put the men upon it and on the rick in fear of their lives. Passing the road, it carried with it a stream of hay, and nearly unroofing a shed on the other side, filled the air to a great height with fragments of hay, leaves, and boughs of trees, which resembled a vast flight of birds in progress across the interval between the London road and Duval's Lane, towards Hornsey Wood. The family of the writer, from his residence a quarter of a mile nearer London, beheld the descending cloud, or water-spout, pass over the spot; and they saw its train, which, at the time, they took to be a flight of birds. They afterwards beheld the descending cloud draw itself upward, and they and other witnesses describe it as a vast mass of smoke working about in great agitation. To them it was nearly vertical, in a northern direction; and to persons a quarter of a mile north it was nearly vertical in a southern direction; and all agree that it drew itself up, without rain, at a short distance to the east of Duval's Lane, and that it was followed near the earth by the train of light bodies. It appeared also, on various testimony, to let itself down in a gradual and hesitating manner, beginning with a sort of knob in the cloud, and then descending lower, and curling and twisting about, till it shortened, and gradually drew itself into the cloud. The inferences, therefore, of the editor, from what he saw and heard, are as follow:

1. That the phænomenon called a water-spout is a mere collection of clouds, of the same rarity as the mass whence they are drawn.

2. That the descent is a mechanical effect of a whirlwind, which creating a vacuum, or high degree of rarefaction, extending between the clouds and earth, the clouds descend in it by their gravity, or by the pressure of the surrounding clouds or air.

3. That the convolutions of the descending mass, and the sensible whirlwind felt at the earth, as well as the appearance of the commencement, increase, and decrease of the mass, all demonstrate the whirl of the air to be the mechanical cause.

4. That the same vortex, whirl, or eddy, of the air, which occasions the clouds to descend, occasions the loose bodies on the earth to ascend.

5. That if in this case the lower surface had been water, the same mechanical power would have raised a body of foam, vapour, and water, towards the clouds.

6. That as soon as the vortex or whirl exhausts or dissipates itself, the phænomena terminate by the fall to the lower surface of the light bodies or water, and by the ascent of the cloud.

7. That when water constitutes the light body of the lower surface, it is probable that the aqueous vapour of the cloud, by coalescing with it, may occasion the clouds to condense, and fall at that point, as through a syphon.

8. That if the descending cloud be highly electrified, and the vortex pass over a conducting body, as a church-steeple, it is probable it may be condensed by an electrical concussion, and fall at that spot—discharging whatever has been taken up from the lower surface, and producing the strange phænomena of showers of frogs, fish, &c. &c.

9. It appears certain, that the action of the air on the mass of clouds, pressing towards the mouth of the vortex as to a funnel (which in this case it exactly represented), occasioned such a condensation as to augment the simultaneous fall of rain to a prodigy.

A water-spout appears, therefore, to be produced by mechanism easily understood. But the writer would ask, whether for important œconomical purposes it may not be possible to imitate this mechanism by erecting hollow cylinders of wood or iron, and exhausting them of air by vessels in communication with them, or by heat, so as to produce the vacuum of a whirlwind, and, by consequence, the condensation and fall of clouds, whenever rain might be urgently wanted for purposes of agriculture?

STEAM-

STEAM-BOAT.

We have omitted in its proper place a note relative to the Plate given with the article on the Steam-Boat, in the present Number. The dotted lines at the side of the paddle-wheels are intended to indicate that the wheels may be made to occupy the whole breadth of the stern of the vessel.

MALVERN WATERS.

A correspondent having requested information respecting the analysis of the Malvern Well, alluded to in p. 231 of our last volume, we insert the result from Dr. Philip's work on this subject, published so far back as the year 1805.

The contents of one gallon of the *Holywell* water are;

Carbonate of soda	5·33 grs.
Carbonate of lime	1·6
Carbonate of magnesia	0·9199
Carbonate of iron	0·625
Sulphate of soda	2·896
Muriate of soda	1·553
Residuum *	1·687
	14·6109

Of the water of St. Ann's Well, Dr. Philip gives the following as the contents of a gallon:

Carbonate of soda	3·55
Carbonate of lime	0·352
Carbonate of magnesia	0·26
Carbonate of iron	0·328
Sulphate of soda	1·48
Muriate of soda	0·955
Residuum	0·47
	7·395

ANCIENT COAL-MINES.

A Dublin paper gives the following account of the ancient coal-mines lately discovered at the Giants' Causeway:

"There were five pits of coal opened in Port Ganneye, west of the Giants' Causeway; the westernmost of which is 244 feet

* This residuum was found to be insoluble in the sulphuric, muriatic, and nitric acids; also in solutions of the alkaline carbonates and of ammonia, and in alcohol; but with the assistance of heat dissolved very rapidly in a strong solution of potash or of soda. The author concludes, that this residuum consists of particles separated from the surface of the glass retort by the action of the water when boiling, and that the soda is the principal agent in producing this separation.

 above

above the level of the sea at half tide, and from thence to the top of the precipice 44 feet.

"In Port Noffer, east of the Giants' Causeway, there were two pits; the westernmost 199 feet from the level of the sea—and from the pit to the top 70 feet. The distance from the first altitude taken at Port Ganneye to that in Port Noffer, is 80 perches.

"The people who found the coal, with difficulty and in some places great danger, threw off the pillars to get at it, and could not pursue it further than cleared, as they had no method of supporting the vast mass above it.

"The stratum of coal dips into the land in a southerly direction; and from the altitudes taken it appears that it lowers as it approaches to the east.

"Several trials at different places have been made to find coal, but none worth following, except under columnar basalt, above which is a stratum of irregular whin-stone, then basalt pillars at the top. The depth of the good seams of coal is from three to five feet; the upper coal, on which the pillars rest, is a soft mossy coal; the wooden coal is in the centre, and the best and more solid at the bottom of the pit. The blocks of wooden coal lie nearly horizontal, in an east and west direction across the face of the promontory. One of those blocks is so large in the east pit, Port Ganneye, that four men with two crow-irons could not turn it out.

"The land, from the precipice to the southward falls considerably."

STEAM ENGINES IN CORNWALL.

The following were the respective quantities of water lifted one foot high with one bushel of coals by twenty-nine engines, reported by Messrs. Leans', in the month of July.

		Pounds of water.	*Load per square inch in cylinder.*
21 common engines averaged		21,077,581	various.
Woolf's at Wheal Vor	..	36,845,637	15·4 lib.
Ditto Wh. Abraham	..	44,987,270	15·1
Ditto ditto ..	..	25,253,888	3·7
Ditto Wh. Unity	..	32,590,596	13·1
Dalcouth engine ..	..	43,028,638	11·2
Wheal Abraham ditto	..	35,089,486	10·3
United mines ditto ..	..	32,094,036	17·9
Wheal Chance ditto	..	37,888,798	13·0

GEOLOGICAL CURIOSITIES AT BOUGHTON HILL.

The workmen employed in cutting through Boughton Hill, Kent,

Kent, have lately found three bullets, nearly thirty feet from the surface, in the solid clay; they are of an oblong form, and the lead is so pure that when cut it exhibits a beautiful metallic lustre; the surface is covered with a green colour, resembling in appearance clay when combined with pyrites. No probable conjecture can be formed as to the manner and time of their deposition; for neither local circumstances, nor the primary stratum in which they were found, can lead to any satisfactory explanation. Several shells have also been found in the secondary stratum, one of which is particularly remarkable, exhibiting in its interior a mass of minute crystals of selenite, which seems clearly to prove that the crystallization of this mineral has taken place subsequently to the deposition of the shell, and therefore may be considered, comparatively speaking, as of recent formation. The fossils are carefully collected by a gentleman in the neighbourhood, and are intended to be exhibited at the cottage on the hill, whenever their number shall be worthy of notice. It is a singular circumstance, that the masses of clay which accidentally fall down exhibit, in every instance which has yet occurred, an inclined plane of 45 degrees—and the surface of these planes, which the workmen call slips, are covered over usually to the depth of a quarter of an inch, with an exceedingly soft species of clay, of a blueish colour. The work on the hill is now going on very well, considering the difficulty which arises from the falling in of the earth at the sides from the want of tenacity in the clayey soil.

NEW BAROMETER.

We understand (says an Edinburgh newspaper) that an instrument has lately been invented by Adie, optician, Edinburgh, which answers all the purposes of the common barometer, and has the advantage of being much more portable, and much less liable to accident. In this instrument the moveable column is oil, inclosing in a tube a portion of nitrogen, which changes its bulk according to the density of the atmosphere. Mr. Adie has given it the name of *sympiesometer* (or measurer of compression). One of these new instruments was taken to India in the Buckinghamshire of Greenock; and by directions of Captain Christian, corresponding observations were made on it and on the common marine barometer every three hours during the voyage. The result, we are informed, was entirely satisfactory—the new instrument remaining unaffected by the violent motion of the ship. We may add, that the sympiesometer may be made of dimensions so small as to be easily carried in the pocket, so that it is likely to be become a valuable acquisition to the geologist.

LECTURES.

London Hospital.—Lectures on the following subjects will be given at this Hospital, to commence in October:

Anatomy and Physiology, by Mr. Headington. Surgery, by Mr. Headington; Midwifery, by Dr. Ramsbottom; Chemistry, by Mr. R. Phillips; Materia Medica, and Pharmacy, by Mr. R. Phillips.

Particulars may be had of Mr. Jenkenson, at the London Hospital.

Mr. R. Phillips will commence a Course of Twenty-four Lectures on Chemistry, at No. 66, Cheapside, on Monday the 6th of October, at Seven o'clock in the Evening.

Tickets of Admission and a Syllabus of the Lectures may be had of Mr. Phillips, No. 1, George-Yard, Lombard-Street, and of Mr. Edenborough, 29, Poultry.

St. George's Medical, Chemical, and Chirurgical School.—The Course will commence in the first week of October, namely:

1. On the Laws of the Animal Œconomy, and the Practice of Physic, (at No. 9, George-Street, Hanover Square,) by George Pearson, M.D. F.R.S., Senior Physician to St. George's Hospital, &c. &c. &c.

2. On Therapeutics, with Materia Medica and Medical Jurisprudence, by W. T. Brande, F.R.S., Professor at the Royal Institution; and by George Pearson, M.D., &c. &c.

3. On Chemistry, at the Royal Institution, by W. T. Brande, Professor of Chemistry, Roy. Inst.

4. On Surgery, by B. C. Brodie, F.R.S., Assistant Surgeon to St. George's Hospital.

5. Sir Everard Home will give, as usual, Surgical Lectures gratuitously to the Pupils of the Hospital.

Anatomical, Chirurgical, and Medical School of St. Thomas's and Guy's Hospitals.—The usual Course of Lectures at these Hospitals will commence in October; viz.

At St. Thomas's.—Anatomy and Operations of Surgery, by Mr. Astley Cooper and Mr. Henry Cline.—Principles and Practice of Surgery, by Mr. Astley Cooper.

At Guy's.—Practice of Medicine, by Dr. Curry and Dr. Cholmeley.—Chemistry, by Dr. Marcet and Mr. Allen.—Experimental Philosophy, by Mr. Allen.—Theory of Medicine, and Materia Medica, by Dr. Curry and Dr. Cholmeley.—Midwifery, and Diseases of Women and Children, by Dr. Haighton.—Physiology, or Laws of the Animal Œconomy, by Dr. Haighton.—Structure and Diseases of the Teeth, by Mr. Fox.

N. B. These several Lectures, with those on Anatomy, and on the Principles and Practice of Surgery, given at the Theatre of St. Thomas's Hospital adjoining, are so arranged that no two of them

them interfere in the hours of attendance; and the whole is calculated to form a Complete Course of Medical and Chirurgical Instruction. Terms and other particulars may be learnt from Mr. Stocker, Apothecary to Guy's Hospital.

The following Course of Lectures will be delivered at St. Bartholomew's Hospital, during the ensuing Winter. To commence October the first:

On the Theory and Practice of Medicine, by Dr. Hue.—On Anatomy and Physiology, by Mr. Abernethy.—On the Theory and Practice of Surgery, by Mr. Abernethy.—On Chemistry and Materia Medica, by Dr. Hue.—On Midwifery, by Dr. Gooch.—Practical Anatomy, with Demonstrations, by Mr. Stanley.

Further particulars may be obtained by application to Mr. Wheeler, Apothecary at the Hospital; or of Messrs. Anderson and Chase, Booksellers, 40, West Smithfield.

Mr. J. Taunton, member of the Royal College of Surgeons of London, Surgeon to the City and Finsbury Dispensaries, City of London Truss Society, &c., will commence his Autumnal Course of Lectures on Anatomy, Physiology, Pathology, and Surgery, on Saturday, October 4, 1817, at Eight o'clock in the Evening *precisely*, and continue them every Tuesday, Thursday, and Saturday, at the same hour.

In this Course of Lectures it is proposed to take a comprehensive view of the structure and œconomy of the living body, and to consider the causes, symptoms, nature, and treatment of surgical diseases, with the mode of performing the different surgical operations; forming a complete course of anatomical and physiological instruction for the medical or surgical student, the artist, the professional or private gentleman.

An ample field for professional edification will be afforded by the opportunity which pupils may have of attending the clinical and other practice of both the City and Finsbury Dispensaries.

Mr. John Mason Good, F.R.S., &c. will commence his Course of Lectures on Nosology, Medical Nomenclature, the Theory, Principles and Practice of Medicine, on Monday, September 29, 1817, at the Crown and Rolls Rooms, Chancery Lane. The Course will rather exceed three months, and be repeated three times a year. From the comprehensiveness of the subject a Lecture will be given every day instead of every other day, as is the usual practice. The Introductory Lecture will commence at Half past Three o'clock in the Afternoon: the subsequent Lectures at Eight in the Morning. The former will be open to the Medical Public, including Medical Pupils, by Tickets, to be had gratuitously at any of the Medical Booksellers of the Metropolis; where the Terms for the Lectures may also be known.

LIST

LIST OF PATENTS FOR NEW INVENTIONS.

To Reuben Phillips, of the city of Exeter, for his new and improved method of purifying gas for the purpose of illumination. —19th July 1817.—6 months allowed for lodging the specification.

To George Wyke, of Bath, and Edward Shorter, of Union-street, Borough, for certain improvements in the construction of wheel carriages.—19th July.—6 months.

To Peter Hamden, of Albany-place, in the parish of St. Giles Camberwell, Surrey, for his improvement or improvements in the making a cement or composition for ornaments and statues, and for making artificial bricks or an imitation of bricks, tiles, and stones, and joining and cementing the same, and for erecting, covering, and decorating buildings internally and externally; and also an improvement or improvements in the mixing, working, and moulding of the said cement or composition upon any sort of materials, or in working and moulding whole and entire erections and substances therewith.—19th July.—6 months.

To Frederick Brunton, of Bride-Lane, Fleet-street, London, for his new mode of employing silk or other materials in the making of hats and bonnets.—19th July.—2 months.

To John James Alexander MacCarthy, of Millbank-street, Westminster, Middlesex, for his road or way for passage across rivers, creeks and waters, and from shore to shore thereof, without stoppage or impediment to the constant navigation thereof, and across ravines, fissures, clefts, and chasms; and a new method or methods of constructing arches and apertures for the running and flowing of water through the same, or under bridges to be used and applied in the construction of the before-mentioned road or way, or otherwise.—28th July.—6 months.

To Louis Felix Vallet, late of Paris, but now of Walbrook, London, for his new ornamental surface to metals or metallic compositions.—5th August.—6 months.

To George Stratton, of Piccadilly, Middlesex, for his method of saving fuel by improvements in fire-places, and more effectually heating and ventilating buildings.—5th August.—6 months.

To Charles Attwood, of Bridge-street, Blackfriars, London, for his improvement or improvements in the manufacture of window-glass of the kind or description commonly wrought or fabricated into crown glass or German sheet glass; and also in a certain process or processes in the manufacture of crown glass. —5th August.—2 months.

To John Hawks, of Gateshead, county of Durham, for his new method

method of making iron rails to be used in the construction of rail-ways.—5th August.—2 months.

To Ludvid Granholm, of Foster-lane, in the city of London, captain in the Royal Navy of Sweden, for his new or improved method or methods, process or processes, mean or means, of preserving such animal and vegetable products or substances separately or mixed together, as are fit for the food of man, for such a length of time as to render them fit for ship and garrison stores. —5th August.—6 months.

To Anthony Hill, of Plymouth Iron-works, for improvements in the working of iron.—5th August.—6 months.

To John Dickinson, of Nash Mill in the parish of Abbott's Langley, Hertfordshire, for his method of manufacturing by means of machinery, paper for copper-plate printing; also paper for writing, drawing, letter-press printing, and of a thicker sort for boards, and similar in texture and substance to card-boards or paste-boards; and certain improvements in his patent machinery for manuafacturing and cutting paper.—5th August.—6 months.

To Dennis MacCarthy, of Little Compton-street, St. Ann's, Soho, Middlesex, for certain improvements on ploughs of various descriptions.—5th August.—6 months.

BRUSSELS PRIZE QUESTION.

The last branch of the second prize question of the Royal Academy of Sciences of Brussels, given p. 380, vol. xlix. has been since amended; and instead of the way in which it is there stated, now runs thus: " In case of no decision, as to the greater probability, which of the two methods of investigating its nature is best calculated to simplify the theory of chemical facts?"

ASTRONOMICAL PHENOMENA, SEPTEMBER 1817.

D. H. M.		D. H. M.	
1. 0. 0	☾ in apogee	14.12. 2	☾ α ♎
2.12.46	☾ A ♉	15. 9. 5	☾ κ ♎
4. 8.54	☾ 125 ♉	15.13.36	☾ λ ♎
6.12.53	☾ ν ♊	15.17. 8	☾ δ ♍
6. 0 0	♀ 20 d1 ♋ * 2′ S.	15. 0. 0	♂ ι ♉ * 20′ S.
7. 1.26	☾ ψ ♋	17. 1 18	☾ θ Ophiuchi
7. 0. 0	♀ ϑ ♋ * 7′ N.	18. 9.42	☾ φ ♐
9. 4 16	☾ η ♌	18. 1.34	☾ σ ♐
10. 0. 0	♀ 44 ♋ * 41′ N.	20. 0 0	♂ 105 ♉ * 23′ S.
11. 0.30	☾ e ♌	21. 9.44	☾ ε ♑
12. 0 0	♀ 374 Mayer * 26′ N.	22. 0. 0	♂ 108 ♉ nearly in contact
12. 0. 0	♀ 380 Mayer * 26′ N.		
12. 2 50	☾ γ ♍	23.22.28	☉ enters ♎
12.16. 1	☾ θ ♍	26.19.51	☾ ο ♓
13. 0. 0	☾ in perigee	29. 0. 0	☾ in apogee
13. 0. 0	♀ 383 Mayer * 24′ N.	29.20.18	☾ A ♉

METEORO-

METEOROLOGY.

At Tunbridge Wells, on the night of Wednesday the 30th of July, about half after eleven o'clock, appeared a beautiful *paraselene*, or mock moon. It was at the distance of about 25 degrees south of the moon, and was highly coloured with red and yellow, and at length had the addition of a projecting and tapering band of light extending in the direction of the halonic radius. The phænomenon lasted about three minutes. The sky was full of the *cirrus* or curlcloud, and the wanecloud passed over in fine veils here and there dispersed in wavy bars. A change had been conspicuous in the clouds to-day. The long lines of *cirrus* extending to either horizon, large well-defined twainclouds to leeward, and waneclouds in the intermediate region of the atmosphere, formed a character of the sky contrasted to the rapid production of rainclouds and showers which had gone on almost every day for a week before.—The barometer was stationary nearly all day, and till midnight, at 29·43.

The Journal of Augsburgh of the 8th ult. has published the following observations made in the Observatory of that city:—"On the 7th inst. at 42 minutes past eight in the evening, Professor Stark observed, in a serene sky, a luminous band, of a colour similar to the Milky Way, in the direction of the head of Serpentarius, in the constellation Hercules; and which passing below the Northern Crown, and then between the tail of the Great Bear, and the head of the Little Bear, ended in the star Alpha of the Dragon. Its length was 71 degrees, and its breadth, almost every where uniform, was two apparent diameters of the Moon. This phænomenon, which had a great resemblance to the prolongation which rapidly took place on the 13th of September 1811, in the tail of the great comet, disappeared at 58 minutes past eight. From this moment until one o'clock in the morning the Professor observed that the nebulous part No. 8, of the constellation of the Buckler of Sobiesky, when the luminous band had commenced, seemed to be surrounded with an *aureola* greater, more lively, and more sparkling than usual.

The great spot or crevice, which appeared on the 23d of July last on the sun's disk, disappeared on the 4th of August. There were afterwards formed a great number of small spots, arranged in several groups, which Professor Stark intends to describe in a work which he proposes to publish very soon.

Meteorological Observations kept at Walthamstow, Essex, from July 15 to August 15, 1817.

[Usually between the Hours of Seven and Nine A.M. and the Thermometer (a second time) between One and Two P.M.]

Date.	Therm.	Barom.	Wind.
July			
15	52 64	29·32	SE—NW—N.NW.—Very rainy; very black *nimbus* 8½ A.M.; sun and great showers all day; stormy; showery.
16	51 62	29·65	N—NW.—Sunshine, *cirrus* and windy; fine day; clear and *cirrostratus*.
17	53 62	29·87	NW.—Clear, *cirrus*, and *cirrostratus*; fine day; rain and wind.
18	53 68	29·87	NW.—Gray and calm; 11 P.M.; wind and *cirrus*; fine day; *cirrostratus* and clear.
19	54 68	99·98	NW.—Clear, clouds, and wind; fine day; moon- and star-light.
20	59 69	30·00	NW—SW.—Clear and *cirrus*; fine day; rain after 6 P.M.
21	58 70	29·88	SW—S.—Clouds and wind; fine day; at 8 P.M. a mackerel *cirrostratus*; clear night. Moon first quarter.
22	61 68	29·77	SW—S—SW.—Rain and hazy; fine day; sun and wind; clear, and *cirrostratus*.
23	59 68	29·78	NW—N—NW.—Gray; showers and sun; great shower at 3 P.M.; clear and *cirrostratus**.
24	60 73	30·00	N—SE—SW.—Sun, and *cirrocumulus*; fine day; clear, and *cirrostratus*; the moon in a *corona*.
25	60 66	30·00	S—SW.—Gray; slight rain; wind, clouds and some sun; clear; *cirrostratus*.
26	60 64	29·99	S.—Gray; slight showers; rainy; clouds and wind.
27	59 57	29·76	SW.—Clear and *cumuli*; 10 A.M. thunder and rain; stormy showers and sun between them; clear, and *cirrostratus*.
28	54 67	29·77	SW.—Clear, clouds, sun, and wind; sunshine; after 5 P.M. storms of rain; star-light. Full moon.
29	58 69	30·00	W—SW—S by E.—*Cumuli*; clear, sun and wind; fine day; star-light.

* July 23d, a man and a dog were killed by lightning at Sevenoaks in Kent; and the steeple of Sunchurch burnt at the same time.

July

July			
30	56	29·77	SE—W.—Sunshine; fine day; *cirrus* and *cumuli*; cloudy.
	68		
31	53	29·77	SE.—Sun, and *cumuli*; sun and showers; storm at Tottenham at 8 P.M.; bright star-light.
	67		
August			
1	51	29·76	W.—Clear and *cumuli*; sunshine, and brisk wind; clear night.
	69		
2	50	29·87	W by S—NW.W.—Clear morning; fine day; cloudy night.
	68		
3	57	29·86	S—SW.—Cloudy and windy; fine day; star-light; 11 P.M. remarkable *cirrocumuli*.
	64		
4	53	29·76	SW—W.—Sun, *cumuli*, and windy; sun and showers; cloudy.
	67		
5	56	29·88	N—SE.—Gray morning and windy; fine day; fine clear star-light night. Moon last quarter.
	68		
6	57	30·10	SW—S.—Sun and *stratus*; clear, and *cumuli*; clear star-light.
	74		
7	56	29·98	SE.—Gray; no sun till about 1 P.M.; clouds; some stars.
	74		
8	51	29·54	SW.—Rain early; showers, sun and wind; fine afternoon; star-light.
	66		
9	55	29·66	NW—W.—Clear, and windy; a shower at noon; fine day; fine star-light night.
	68		
10	57	29·78	W—SW—NW.—Slight showers, and sun, and wind, hazy and sun; showery; clear star-light.
	66		
11	56	29·77	S.—Fine morning; sun and clouds; gray day, but some sun about 3 P.M.; slight showers after 6 P.M.; cloudy.
	69		
12	55	29·43	S.—Sun, wind, and hazy; shower at noon; fine day; star-light; rain 10 P.M. New moon.
	68		
13	58	29·32	SW.—Cloudy and great wind; great showers; sun and wind; star-light.
	68		
14	60	29·66	SW—S.—Rain, sun, and windy; sun and clouds, and windy; showers all day; mottled *cirrostratus* at 6 P.M.; rain, and very dark.
	70		
15	60	29·66	S—SW.—Sunshine; fine day; some drops of rain; star-light.
	66		

The 19th of last June, the 2d time of the Thermometer was 70, and that was at 8 A.M.; it was taken again at 3 P.M. and was then 80, as it was *unavoidably* missed that day at the usual time.

METEORO-

METEOROLOGICAL JOURNAL KEPT AT BOSTON, LINCOLNSHIRE.

[The time of observation, unless otherwise stated, is at 1 P.M.]

1817.	Age of the Moon	Thermometer.	Barometer.	State of the Weather and Modification of the Clouds.
	DAYS.			
July 15	1	54·	29[illegible]	Heavy rain all the day
16	2	58·5	29·[illegible]	Fair—some rain P.M.
17	3	60·5	29 94	Ditto ditto
18	4	63·	29·97	Showery—heavy rain P.M. till the next morning
19	5	61·	30·03	Fair
20	6	62·	30·05	Ditto
21	7	70·	29·90	Ditto—some rain A.M.
22	8	67·	29·87	Ditto ditto
23	9	[illegible]	30·05	Ditto ditto P.M.
24	10	70·	30·15	Ditto
25	11	67·	30·06	Ditto
26	12	66·	29·83	Ditto
27	13	62·	29 66	Showery
28	full	64·5	29·90	Fair
29	15	68·	30 06	Ditto—rain in the even^{g} and night
30	16	70·	29·88	Ditto ditto
31	17	66·	29·77	Thunder storm—heavy rain
Aug. 1	18	57·	29·90	Showery
2	19	61·5	30·05	Fair—rain in the evening
3	20	62·	29·72	Showery
4	21	60·	29·80	Ditto
5	22	63·	30·1[illegible]	Fair through the day
6	23	69·	30·14	Ditto ditto
7	24	66·	29·95	Ditto
8	25	57·	29·54	Stormy—rain
9	26	62·	29·72	Showery
10	27	63·	29·91	Fine all the day
11	28	62·	29·80	Fair—heavy rain at night
12	29	66·	29·50	Ditto—gale from the W.
13	new	63·	29·49	Ditto ditto
14	1	68·	29·77	Showery ditto

The harvest in this neighbourhood will not commence generally for at least fourteen days.

METEORO-

METEOROLOGICAL TABLE,

By Mr. Cary, of the Strand,

For August 181[illegible]

Days of Month.	Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock, Night	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
July 27	61	66	55	29.62	36	Showery
28	60	66	59	·60	42	Showery
29	60	67	[illegible]	[illegible]	48	Showery
30	60	68	[illegible]	·72	58	Fair
31	58	66	54	·71	42	Showery
Aug. 1	57	69	55	·75	51	Showery
2	55	69	59	·69	65	Fair
3	55	66	55	·70	54	Fair
4	59	68	56	·72	55	Fair
5	58	67	59	·95	57	Fair
6	60	72	58	30·00	65	Fair
7	58	73	57	29·79	72	Fair
8	59	65	55	·50	46	Showery
9	60	[illegible]	55	·70	42	Showery
10	57	68	56	·80	40	Showery
11	59	65	60	·65	[illegible]	Cloudy
12	58	60	58	·45	[illegible]	Showery
13	[illegible]	[illegible]	59	·51	48	Showery
14	62	68	57	·67	35	Cloudy
15	60	68	55	·78	62	Fair
16	60	69	56	·75	52	Fair
17	58	62	55	·80	42	Stormy
18	57	65	60	·92	43	Cloudy
19	60	[illegible]	60	·68	36	Showery
20	60	68	55	·60	50	Fair
21	55	58	52	·80	25	Stormy
22	54	60	53	30·06	47	Fair
23	[illegible]	64	56	29·90	46	Fair
24	56	60	56	·56	22	Showery
25	59	57	52	·16	[illegible]	Rain
26	55	62	53	·01	15	Stormy

N.B. The Barometer's height is taken at one o'clock.

ERRATUM.—In Sir RICHARD PHILLIPS's paper, in this Number, at the end of the 2d objection, for "being the orbicular force common," read "the orbicular force being common."

XXIV. *On Iodine.* By Andrew Ure, M.D. *Professor of Chemistry, &c. &c., Glasgow.*

To Mr. Tilloch.

Sir, — The great trouble and uncertainty attending all the processes which have been prescribed in the scientific journals for procuring this interesting elementary body, and the high price at which it is sold in Great Britain, induced me about two years ago to inquire whether an easier and cheaper mode of preparing it might not be discovered*.

As many of the Scotch soap manufacturers use scarcely any other alkaline matter for their hard soaps except kelp, it occurred to me that in some of their residuums a substance might be found, rich in iodine. Accordingly, after some investigation, I found a brown liquid of an oily consistence, from which I expected to procure what I wanted. This liquid drains from the salt, which they boil up and evaporate to dryness from their waste leys for the soda manufacturer. I instituted a series of experiments on the best mode of extracting the iodine. As these succeeded far beyond my expectation, I hope the following account of them will prove not uninteresting to the British chemist.

The specific gravity of the above liquid, as obtained at different times, is very uniformly about 1·374, water being 1·000. It converts vegetable blues to green, thus indicating free alkali. Of this the manufacturer is aware, for he returns it occasionally into his kelp leys. Its boiling point is 233° Fahr. Eight ounces apothecaries' measure require precisely one measured ounce of sulphuric acid for their neutralization. Supposing this quantity of acid combined with soda, it would indicate one part of pure soda in eleven by weight of the liquid. But the greater part of the alkali is not uncombined; for an immense quantity of sulphurous acid and a little sulphuretted hydrogen gases escape on the affusion of the sulphuric acid. One hundred grains of the liquid yield 3·8 cubic inches of gas, chiefly sulphurous acid; and sulphur is at the same time deposited. From the quantity of sulphur, one might expect a larger proportion of sulphuretted hydrogen; but the disengaged gas possesses the peculiar smell and pungency of burning sulphur, blanches the petals of the red rose, but shows hardly any action on paper dipped in saturnine solutions. In the instant of decomposition of the sulphite of soda, and hydroguretted sulphuret existing in the liquid, the nascent sulphurous acid of the former may be supposed to act on the

* The iodine sold in London is for the most part imported from Paris, as I was informed by an eminent practical chemist.

nascent sulphuretted hydrogen of the latter; their atoms of oxygen and hydrogen uniting to form water, while the sulphur of both is precipitated. I cannot in any other way account for the very copious separation of sulphur, while very little sulphuretted hydrogen appears. From the excess of sulphite present in the liquid, we have a redundant quantity of sulphurous acid evolved. From eight liquid ounces, equal by weight to eleven, 213 grains of sulphur are obtained.

The liquid saturated with the sulphuric acid has a specific gravity of 1·443, a bright yellow colour, and it does not affect the purple infusion of red cabbage. I distilled eight ounces of this in a glass retort. The stopper of the tubulated receiver was frequently blown out by the escape of incondensable gas, even after the liquid had been for a long time in ebullition. This, which was probably hydriodic acid gas, continued to be evolved to the very last. In the receiver, which had been kept very cool, a colourless and nearly transparent liquid was found. Its specific gravity was 1·054, of an acidulous and acerb taste; it reddened vegetable blues, and powerfully blackened brass. From this liquid I could extract only three or four grains of iodine, though the viscid black substance left in the retort yielded more than twenty times the quantity. We see therefore that by distillation very little hydriodic acid can be procured from the saturated liquid.

In the prosecution of my researches to ascertain the best mode of extracting the iodine, I at length discovered the causes of the anomalous results which had not a little perplexed me at first, rendering the product very uncertain. The following method was found to answer extremely well.

The brown iodic liquid of the soap-boiler was heated to about 230° Fahr.; poured into a large stone-ware bason, of which it filled nearly one-half, and was then saturated by the proper quantity of sulphuric acid, as above stated. The acid ought to be previously diluted with its own bulk of water*. On cooling the mixture, a large quantity of saline crystals is found adhering to the sides and bottom of the vessel. These are chiefly sulphate of soda, with a very little sulphate of potash, and a few beautiful oblong rhomboidal plates of hydriodate of soda. The precipitated sulphur is intermixed with these crystals.

After filtering the cold liquid through woollen cloth, I add to every twelve ounces apothecaries' measure, 1000 grains of powdered black oxide of manganese. This mixture is made in a glass globe or matrass, over the mouth of which a glass globe is

* When concentrated oil of vitriol is added, the effervescence is very violent; the liquid reddens wherever the acid falls, and a little of the purple vapour of iodine rises.

then

then inverted. The heat of a charcoal chaffer being now applied, the iodine sublimes in great abundance. To prevent the heat from acting on the globular receiver, a thin disc of wood, with a round hole in its centre, is placed over the shoulder of the matrass. As soon as the globe becomes hot, another may be substituted in its place; and thus two or three may serve in rotation to condense a very large quantity. The iodine is easily washed out by a little water. It is then drained on glass plates, and dried. From the above twelve ounces of liquid I usually obtained about 200 grains of iodine. This may be purified by a second sublimation from dry quicklime. The most convenient apparatus is that represented (Plate III. fig. 1.) It is composed of an exterior vessel *b*, containing the mixed materials, and an interior one *a*, filled with cold water. On the outside of *a*, beautiful large crystals concrete, and by lifting up *a* they may be readily detached without breaking them. If in the operation of subliming the water of *a* should become hot, it is easy to run it off with a siphon, and to fill it again with cold, or to put into it some ice. I have not seen any such apparatus described before, and I can recommend it as possessing many advantages over the subliming vessels usually employed.

If the manganese be increased much beyond the above proportion, the product of iodine is greatly lessened. If, for example, thrice the quantity be used, a furious effervescence ensues; nearly the whole mixture is thrown out of the matrass with a kind of explosive violence; and hardly any iodine is to be procured, even though the materials should have been saved by putting them into a very large vessel. On the other hand, should only one-half of the prescribed quantity of manganese be used, much hydriodic acid rises along with the iodine, and washes it perpetually down the sides of the balloon. Or, if during the successful sublimation of iodine the weight of manganese be doubled, the violet vapours instantly cease. Neither sugar nor starch restores to the mixture the power of exhaling iodic vapour.

A similar interruption of the process is occasioned by using an excess of sulphuric acid. For, if to the mixture of twelve ounces of saturated liquid, and 1000 grains manganese, an additional half-ounce measure of sulphuric acid be poured in, the violet vapour disappears, and the sublimation of iodine is finally stopped. Quicklime, added so as to saturate the excess of sulphuric acid, does not renew the process. In these two different cases, iodic acid is probably formed by the too rapid and copious supply of oxygen. For the due decomposition of hydriodic acid, the oxygen ought to be afforded merely in the quantity requisite to saturate its hydrogen.

The best subliming temperature is 232° Fahr.; though in open

vessels it readily evaporates at much lower degrees of heat, even at that of the atmosphere. When it is spread thin on a plate of glass, if the eye be brought into the same plane the violet vapour is discernible at 100°. It evaporates slowly in the open air at 50° of Fahrenheit. When put into a phial closed with a common cork, the iodine soon disappears, it combines with the substance of the cork, tingeing it brownish yellow, and rendering it friable.

240 grains of nitric acid, sp. gr. 1·490, saturate 1000 grains of the iodic liquid. Sulphurous acid is copiously exhaled as before. After filtration a bright golden-coloured liquid is obtained. On adding a little manganese to this liquid, iodine sublimes; but the quantity procurable in this way is considerably less than by sulphuric acid.

I am, &c.

Anderson's Institution, Glasgow, August 29, 1817. ANDREW URE.

XXV. *Theorems for determining the Values of increasing Life Annuities. By Mr. J. B. Benwell.*

To Mr. Tilloch.

SIR, — The following collection of theorems embraces an extension of those communicated in a previous Number of your Magazine, being applicable to the valuation of life annuities increasing by certain orders of a constant numerical ratio.

The several Life Assurance Companies established in the metropolis are occasionally in the habit of granting annuities that increase by the scale of the natural numbers as well as the multiples thereof, and which annuities may be either temporary or deferred; but, in respect I (presume) to those institutions which do not possess the proper and requisite aids (in conducting this branch of scientific research), it has been represented as a matter of much apparent doubt, whether the methods they pursue, in order to arrive at the supposed values in these and similar inquiries, be rigorously exact and unobjectionable — a circumstance that imperiously requires elucidation, because it tends to militate against the avowed professions held out by them, of being guided by the pure and unerring principles of mathematical truth. It is very probable that the practice of granting progressive life annuities might be rendered almost as general as any other species of contingent investment; and what seems chiefly essential to the dissemination thereof, is a commodious and accurate formula for the solution of the most useful cases. But with the exception of one for finding the value of a life annuity, increasing according

to the common scale of notation, (as given in most treatises on the subject,) no others for this purpose, I believe, exist any where in print, but in the present work; in regard to which I have only to observe, they are as simple and concise as the nature of the investigation would possibly admit: and as simplicity and accuracy were objects indispensably in view, so they have not been attained without some efforts of patience and perseverance. My studies are prosecuted under auspices the most unfavourable: I have to lament that my present situation but so ill accords with a disposition for scientific pursuits.

The several formulæ I shall enumerate will apply in the four following cases; viz. when the annuity increases in the order of the numbers (1. 3. 6. 10. 15) (1. 3. 5. 7. 9) by the squares of this latter series, and also by the squares of the series (1. 2. 3. 4. 5.)

Then in the first case the formula exhibiting the value of the annuity will be

$$\frac{\ldots(1-x) + 6(a+1) + \overline{x-1}.(a^2+9a+10)x^{-a} + \overline{x-1}|^{2}.(a^2+3a+2)x^{-a}\left(\frac{2+3x-1}{x-1}.2.(1-x) + \overline{x-1}\ldots\right)}{2a.(x-1)^3}$$

In the second:

$$\frac{a.(x+1) + (2a+5+\overline{x-1}.(2a+1))x^{-a} - \left(\frac{4}{x-1}.(x^{-a}-x) + x\right)}{a.(x-1)^2}$$

And for the two succeeding cases it will be respectively,

$$\frac{\ldots x) + \overline{x-1}|^{2}.a + (4.(6a+8) + \overline{x-1}.(4a^2 + [illegible] 17)x^{-a} + (x-1)^2(2a+1).x^{2-a}\left(\frac{6 [illegible]}{x-1}.4.(1-x) + 16.(\ldots\right)}{a.(x-1)^3}$$

And,

$$\frac{\frac{1}{\ldots-1}.(6(a+[illegible]\,(a^2+6a+7)x^{-a} + \overline{x-1}.(a^2+2a+1)x^{-a} + a - 1.x - \left(\frac{6x-(x-1.2a)}{(x-1)^2}.(x^{-a}-x)\right)}{a.(x-1)^2}$$

In each of the foregoing formulæ x denotes the ratio or amount of one pound for a year; and (a) the complement or double the expectation of human life, according as it is deduced from any assigned table of observations.

The annuity may commence with the addition of some fixed annual payment, still increasing in the same order, as for instance (11. 13. 16. 20) (11. 13. 15. 17), and so on, for the other series; and thus may be generated various forms of increasing annuities at pleasure. In this case the only difference will be, that we must augment the value previously obtained, by the value of such additional annuity on the given life for the total value required.

If the annuity, being a deferred one, does not commence until a given period equal n. years, the quantity (a) must be deter-

 mined

mined accordingly; that is, from a life n years older; and after the proper substitution is made, the result afforded by each particular formula must be combined with the numerical value of the expression denoting the expectation of the given life receiving 1*l*. n years hence for the value of the annuity in this case.

Having thus found the value of an annuity deferred for n years, we may thence derive the value of a similar temporary annuity depending on the given life continuing so long in existence.

I much wish that I could have represented these different formulæ by others involving the combination of the equal single and joint lives. But in each particular instance here adduced this object could not well be accomplished.

I shall here introduce the expressions for the sums of a few other series that occurred in the course of investigation, and which may be found useful on some occasions.

Let X represent

$(x^{-1}+5x^{-2}+9x^{-3}+13x^{-4}+17x^{-5}+\ldots.(4n-3)x^{-n})$.

Y $(x^{-1}+5x^{-2}+12x^{-3}+22x^{-4}+35x^{-5})\ldots(\tfrac{1}{2}3n^2-1)x^{-n})$.

And, Z $(x^{-1}+17x^{-2}+57x^{-3}+121x^{-4}+209x^{-5}+\ldots.(12n^2-20n+9)x^{-n}$. Then will the general expression for the sum of each series be respectively,

$$X=\frac{\frac{4+x-1}{x-1}(1-x^{-n})-4nx^{-n}}{(x-1)}.$$

$$Y=\frac{\frac{(2+3\overline{x-1})}{x-1}(1-x^{-n})+x+\overline{x-1}^{\,1}.(1-x^{-n-1})-(3n\ [illegible]\ 3n^2+5n+2))x^{-n}}{(x-1)^2}.$$

$$Z=\frac{\frac{(6+2\overline{x-1})^{-1}}{x-1}.4.(x^{-1}-x^{-n})+16.(1+x^{-1})+x-1-(4(6n+2)+\overline{x-1}.(12n^2+4n+1))x^{-n}}{(x-1)^4}.$$

In regard to the practical enunciation of the above theorems, and generally of any other for summing reciprocal series of this kind, where the terms of such series are very large, and the rate of increase also rapid, it may be observed, that the negative powers of (x) should be expanded to a proportionally greater extent, in order to obtain a result perfectly accurate.

The facility and marked attention with which my preceding communications were inserted in the Philosophical Magazine, have encouraged me to a further prosecution of these subjects; and I intend at a future opportunity (should I find means—inclination I possess) to furnish you with a paper embracing the discussion of some interesting and rather novel points in the doctrine of life assurances.

Haberdashers-Place, Hoxton, Aug. 15, 1817.

JAS. BENJ. BENWELL.

P. S. In my former communication for April, when stating the equation which has (although improperly) been made the principle and derivation of the common rule for equating of payments, I purposely withheld the following note, with some additional observations, but which circumstances have not rendered necessary;—still however the insertion of the note is essential, as affording perhaps a more simple and decisive confirmation of the truth of the above rule.

Since $(b.r.t)$ is the whole accretion derived by A. for the term (t), so collaterally will $(a.r.t)$ be that derived by B. in the like interval. Now these objects being jointly effected by the rule (as they ought to be), we need only conceive x. to have such a value that $br.(t-x)$ the gain of B in $(t-x)$ time shall equal $(a.r.x)$ his loss by the detention of the sum (a) for the time x. Yet on the other hand it may be urged, that $(a.r.x)$, the gain on a in x. time is equal $br.(t-x)$ the loss on b. for $(t-x)$; and therefore $(b.r.t)$, the whole interest must be actually made in such time. Now this equating interest with interest in place of discount certainly seems erroneous; but discard the restriction imposed or applied in this case [that of $(a+b)$ instead of $\left(a+\frac{b}{1+rt}\right)$, being the sum in hand at the end of the first term], the difficulty then vanishes, and the thing appears, what it really is, simply a deduction or corollary from the general expression and indicating an equality between those quantities, but which can have no absolute relation to or dependence on the conditions constituting the right and interest which arises in the question.

XXVI. *Report of the Select Committee appointed to consider of the Means of preventing the Mischief of Explosion from happening on board Steam-Boats, to the Danger or Destruction of His Majesty's Subjects on board such Boats.*

[Continued from p. 100.]

Mr. WILLIAM CHAPMAN'S *Evidence.*

WHAT is your profession, and place of abode?—My profession is civil-engineer; my general place of abode is Newcastle-upon-Tyne.

Have you, as engineer, turned your attention to the construction of steam-engines for steam-boats?—As to steam-boats I have not particularly; but I have been concerned in steam-engines of every description, from being connected with the collieries, where we have many engines.

Have you any steam-boats upon the Tyne?—We have.

Have you seen those steam-boats?—Yes; I have.

 How

How many have you?—I think it is three; but I have only been in one of them.

Do you know the construction of the steam-boats employed upon the Tyne?—Low pressure condensing engines.

Are you aware of any reason which would render it expedient to forbid the use of high pressure engines on board steam-boats?—I look upon all engines, whether high pressure or low pressure, as dangerous to the passengers, unless due precaution be taken to emit the steam when exceeding a given pressure; for in low pressure engines the boilers are always liable to burst or to alter their form, when the pressure becomes superior to the resistance; all boilers but those that are cylindrical in the section, and with hemispherical ends or portions of spheres or cones or conoids, are liable to alter the form by the natural expansive force of the steam, and therefore all boilers but of those forms owe their safety to their weakness; because if weak they will alter their form without danger, and if strong, they have been known to bend the iron so abruptly as to break asunder.

Are you speaking of wrought, or cast iron?—I speak of wrought iron; and consequently they explode, and in many instances have destroyed several of the passengers; they are so far more dangerous to the passengers that they frequently scald them, and do not actually kill them. There are a description of engines in use in the counties of Durham, Northumberland, Cumberland, and York that are termed loco-motive engines; the form of their boilers is cylindrical, with curved ends.

Are those applicable to boats?—Certainly; they are high pressure engines working with a force of from fifty to sixty-five pounds per inch; and no accident has happened to any of them but to one, the safety valve of which was stopped by a man sitting upon it or holding it down purposely; he said, "We will have a good start and surprise them, we will go off so well;"—the consequence was, that the boiler blew up and killed and wounded a very considerable number of people; I believe to the extent of forty-five, but I am not certain.

Was that a cast- or a wrought-iron boiler?—It was wrought iron.

Can you suggest the means by which a high pressure engine can be rendered safe on board a vessel?—It can only be rendered safe by having the form of the boiler, such as I have described, and the cylindric part of a limited diameter, with a competent thickness of wrought metal, either iron or copper, and the plates secured to each other by a double line of rivets; it is also requisite that there should be two safety-valves, each laden with any determinate weight per superficial inch of the narrowest part of the seat of the valve; one of those valves should be at perfect liberty

liberty to be raised at the pleasure of the manager, because sometimes it is expedient to raise it; the other should be under a cover of such description as not to be opened at all, at the discretion of the engineer, but with sufficient apertures for the emission of the steam, and for any of the passengers to see that the valve is not made fast. It is also requisite that there should be a mercurial gauge of not less than an inch in diameter, and whose longest limb shall not be greater than two inches and one-eighth for every pound per inch upon the safety-valve; it is necessary, by occasional inspection, to take care that the mercury does not stiffen by oxidation, occasioned by the heat and motion to which it is in a slight degree liable.

Do you conceive that a high pressure engine thus guarded might be used on board a steam-boat with safety to the passengers?—Yes, so long as the boiler is kept in order; but the boiler's bottom is liable to erode or consume by the action of the fire, and therefore requires watching.

How long do you think a boiler would last under the action of fire?—A boiler may last twelve months safely, provided its bottom be made of charcoal iron, beat not rolled, because there is a great deal of difference in the grain.

Would you not always recommend a boiler to be made of wrought metal on board steam-boats?—On board steam-boats I would recommend them all to be made either of copper or charcoal iron plates beat under the hammer and not rolled; the resistance of cylindric boilers will be in the inverse ratio of their diameters.

[Mr. William Chapman was again called in on a future day, at his own request, and stated, that when he was asked as to locomotive engines, he omitted to say that the diameter of their boiler was in general four feet, little more or less; that many of them are formed of cast iron, and several of malleable iron, and that the ends of several of these latter are of cast iron curved outwards; that in no one of them does the fire act upon the external part of the boiler, but is placed in a malleable iron tube which passes through the boiler; a cast iron boiler, however, being found far too heavy, the new loco-motive engines are always supplied with malleable iron boilers.]

Mr. Philip Taylor's *Evidence.*

Will you be so good as to state what is your occupation?—A manufacturing chemist.

Where do you reside?—At Bromley in Middlesex.

You are conversant with the nature of steam-engines?—My attention has been directed to the use of steam from a desire to apply

apply it in my own business, not as a moving power, but for the purpose of communicating heat to different fluids, for which purpose I have required high pressure steam. I have a patent for a mode of applying high pressure steam to vessels of the largest capacity; and as in this case all danger depends on the construction of the boiler, I should wish to say a little on those boilers which I have found to be the most trust-worthy. I come quite unprejudiced as to any material, and as to any form; for if I could meet with a boiler which would answer the purpose I have in view better than that now used by me, I would adopt it; therefore I shall give the Committee only such facts as have come within my own immediate knowledge. I have no wish to recommend any particular construction.

Will you be so good as to state from your knowledge, what species of boiler for a high pressure engine you would recommend in regard to safety?—I consider the first and most material point to attend to in the construction of high pressure boilers is, that the diameter of such boilers should be small in proportion to their capacity; and that as small a proportion of the external surface of the boiler as possible should be exposed to the destructive action of the fire; and that the portion of the boiler so exposed, should be so situated and guarded, that in case of explosion the least possible mischief would arise. In those boilers which I have made use of, no portion of the boiler is exposed to the action of the fire without its being constantly covered with water; and the fire is applied under an arch of not more than two feet and a half in diameter; this provides against any extensive rent taking place in the event of explosion. The boilers I have generally employed are constructed of malleable iron, commonly known by the name of charcoal iron, riveted together and secured by strong wrought-iron belts. From observing the danger arising from the introduction of flat cast-iron ends, I have terminated the ends of the boilers by wrought-iron ones nearly hemispherical; this mode of construction, as far as my experience goes, combines more strength and durability than any other. The precautions I have made use of to guard against the misuse of such boilers, have been by adapting to them two safety-valves; one under the control of the engine-man, the other secured in a strong cast-iron case, locked down and loaded with such a weight as would suffer the steam to escape when it had arrived at an improper degree of expansive force. In order to add to the security given by safety-valves, I have likewise in every instance attached to the boiler a mercurial column, the bore of which is proportioned to the size of the boiler; and I should consider an iron tube of an inch diameter sufficient to

guard

guard against accident, when applied to a boiler four feet in diameter and twenty feet in length, because the limit given by such a column has always been far within the limit of absolute safety. The length of the external limb of the mercurial gauge has in all cases been proportioned to the strength of the boiler and the force to be applied, taking care that the expansive force of the steam would displace the mercury long before any dangerous expansive force would arise. In order to guard against the boiler's being injured by the action of the fire, from a deficient quantity of water in the boiler, I have inserted a leaden rivet in such a situation that it would melt as soon as it was uncovered by the water, and produce an opening which would suffer the escape of the steam. Although I have made use of boilers of this construction, I consider cast-iron boilers safe, provided their various parts are made of small diameters in proportion to their capacity; such for instance as those constructed by Mr. Woolf.

From your knowledge of the subject, do you think you can take upon yourself to say, that a high pressure engine with a boiler constructed on the principles you have just now detailed, would be completely safe for the use of passage-boats? — I think equally safe with those called condensing engines, because a greater attention to strength is always paid in the construction of high pressure boilers than in the construction of low pressure boilers, in proportion to the pressure they have to sustain.

Have not very great improvements been recently made in high pressure engines, by which the general mining and manufacturing interests of the country have been greatly benefited?—I think very important ones: the high pressure engine, as constructed by Mr. Woolf, employs not only the expansive force of the steam, but also that power which is acquired by its condensation; and the effect in Cornwall has been, that engines on this construction have done double the quantity of work with the same quantity of fuel.

Does your own experience lead you to conclude, that the high pressure engine in general is less expensive in point of consumption of fuel?—If well constructed they are decidedly œconomical engines with regard to the consumption of fuel.

You mean then by this, that the advantage of the high pressure in point of œconomy in fuel is not confined to engines of any one particular construction?—Certainly; I mean it is not so confined.

Have you any connexion whatever with Mr. Woolf?—None whatever; I am not personally known to him.

Have you any reason to suppose that the high pressure steam-engines are already arrived at the degree of perfection of which they

they are susceptible?—Certainly not; Mr. Woolf's engine has been much simplified since its first invention, and my opinion is, it will be still further improved.

You would then consider any measure which should tend to impede the use of high pressure engines to be injurious to the country?—Certainly, I should.

Mr. Henry Maudeslay's *Evidence.*

What is your profession?—I am an engineer, residing at Lambeth.

You construct steam-engines?—Yes, a great many.

Are you at all acquainted with the circumstances attending the explosion of the steam-engine at Norwich?—Yes, I am.

Have you been there since this accident?—No.

Did you know the steam-boats there before the accident?—Yes; because I made a steam-boat for Yarmouth.

Was the steam-boat you made, a high pressure or a low pressure engine?—A low pressure engine.

Will you be so good as to tell the Committee, what is your opinion with regard to the proper construction of those engines, to secure the passengers on board those boats?—I never considered high pressure engines were applicable to boats, because the purpose of a high pressure engine is to save water, and water cannot be wanted on board a vessel; the difference between the one and the other makes no saving either in the weight or expense, taking it ultimately, particularly when steam-boats are properly contrived. As far as my opinion goes as to steam-engines and steam-boats, I would not go from here to Margate in a high pressure boat, because there are many reasons why that may become much more dangerous, and no more advantageous to the public generally or to the individuals. A low pressure engine is of very high power; a high pressure engine has a higher power in proportion to its height of steam. It is pretty well understood, that a gentleman who engages in a steam-boat company seldom attends to the engine himself, but leaves it to his men. I built the Regent steam-boat last summer with a low pressure engine; there was a dispute between two men, and one of them swore that he would blow his boiler up, but he would beat the Regent in coming up. The man certainly did exert himself as much as he could, and kep this steam as high as he could get it, and it flew out of the safety-valve very frequently, and he hurt his boiler materially from doing so, but he did not beat the Regent; but if it had been a high pressure engine, he would either have beat her or blown up his boiler, because he had the power in his own hands.

Had it been a high pressure engine, and the boiler properly constructed,

constructed, with sufficient safety-valves adapted to it, the operation of which the man could not impede, would it have been liable to accident?—I feel some difficulty in answering such a question as that, because I am afraid that there are so many technical terms in engine-making, and reasons why safety-valves should be attended to, that I doubt whether they would not go to more evil by the man not having access to them than by their being open to him.

If there was one safety-valve which was not accessible to the engineer, and another which was, would not that danger be prevented?—I would beg to explain, by saying, that on board the Regent, which has a large boiler, I found it necessary to have two safety-valves, and sometimes I put three safety-valves: to make it quite easy for the man to move the valve, I have a sort of bell-pull going down to the place where he stokes, to pull it up every hour if he pleases, to keep it in action, because it is clear the spindle may corrode and stick fast for want of use. Supposing it not touched once a week, it is not a safety-valve any longer, because a very little friction will add a great many pounds weight to the opposition the steam ought to meet with.

According to your experience and knowledge, would a low pressure engine be safe in most cases that can occur?—I never knew a low pressure engine unsafe, but it appears that high pressure engines have been.

Would a high pressure engine, under the same circumstances, be equally safe?—Certainly not.

Do you conceive there is any difficulty in constructing a safety-valve in such a manner as that the engineer shall be able to keep it in constant fitness for its action, without having a power to fasten it down and prevent it from acting?—I conceive that the same motive which would induce the engineer to work it with an improper pressure, would induce him to leave it untouched, that it might have an improper pressure. I beg to state, that there is not that difference between a high pressure and a low pressure engine, as to its power, that is generally supposed; because it is understood, that the steam in the boiler is kept at from four to six pounds upon the inch, but from two and a half to four is quite abundant for any use a low pressure engine can be wanted for: then, if an engine is in any thing like working order, there is a vacuum formed by the engine itself, by the construction, that causes an addition of ten pounds the inch. On the lowest calculation, those two added together, make fourteen pounds; if you take high pressure steam at forty pounds the inch, you do not, in my opinion, get additional force in proportion to the risk incurred; because we well know, that if the boiler be of cast iron, faults will unavoidably arise in casting which you cannot

see, which cause explosions or breakings, and which could not be calculated upon.

Is there any thing which prevents the engineer from fastening down or over-weighting the valves of a common condensing engine?—It would be folly for him to do so.

Is there any thing which prevents him?—Certainly not.

Supposing the valves to be so fastened down, does not the engine immediately become unsafe?—Yes, certainly it must; but it would be folly to fasten it down, because, if the engineer be at all acquainted with his business, he must know, that if the steam be raised beyond five or six pounds per inch in a condensing engine, the power of the engine will not thereby be at all increased; the condensing property of the engine does not consist in a higher pressure of steam.

What is your opinion as to the comparative safety of cast and wrought metal used in boilers?—I consider that wrought iron is extremely safe, compared to cast iron.

Then at all events, it is your opinion, that in steam-boats boilers of wrought metal should be used in preference to cast?—No doubt about it.

Do you think there is any material difference between the use of copper and wrought iron?—No, excepting in the greater degree of corrosion to which iron is liable.

Are you aware of there being any considerable difference in the consumption of coals, necessary to produce any given power in condensing and high pressure engines?—I consider that the one will work with as little coal as the other; in all high pressure engines and condensing engines I have heard of, I find little or no difference, and those who have them tell me they burn as much coal in the high pressure engine as in the low pressure engine: I have understood that Woolf's engine does save coal.

Do you know that to be the fact?—I do not, because I never attended any experiments; but I have heard it from so many people that I cannot but believe the fact is, they save coal.

If a high pressure steam-engine had a wrought-metal boiler, either of iron or copper, constructed by a competent engineer, with safety-valves in proper order, and a mercurial gauge, should you then think yourself in any danger in a steam-boat propelled by such an engine?—Certainly not, if a competent person had the superintendence of it.

Mr. Alexander Galloway's Evidence.

What is your profession and place of abode?—I am a mechanist and engineer, residing in Holborn.

Do you know any thing of that paper [*showing a paper to the witness*]?—I have seen it.

Do

Do you know by whom it was published?—I do not know; I have heard it was done by the proprietors of some of the steam-boats; the letter I wrote was sent to the Morning Chronicle; it was only within the last three days I saw it in that form.

Have you been employed at all in constructing steam-engines for steam-boats?—I have not.

Were you acquainted at all with the accident at Norwich?—No; but what I have heard.

The object of this Committee being to insure the safety of the passengers on board steam-boats, will you favour them with your opinion as an engineer, what means are best adapted to insure that safety?—I should certainly recommend, that, for steam-boats, the condensing engines should be used in preference to high pressure engines, and I will give you my reasons why I do so. In the first place, the great advantage that has been promised from a high pressure engine is, that it can be worked in a situation where water cannot be procured, and therefore, under this circumstance it is for such a situation a valuable machine; but in situations where water can be readily procured, it is not so. And in reference to the comparative price between a high pressure engine and a low pressure engine, and in reference to the space that it occupies, and in reference to the superintendence that it requires, I am decidedly convinced no œconomy is produced. Speaking to it as a matter of safety, it will be necessary for me to say, that experience has fully proved, that the maximum of force to be obtained by a condensing engine, is when the steam is rarefied from three to six pounds on the inch; the engine is by far more efficient than when the steam is rarefied up beyond; and it will appear equally clear, that whether it be a cast-iron boiler or a wrought-iron boiler, or a copper boiler, the force of the engine is better performed by steam at three pounds and a half, than it is at any increased expansive force; the boiler being subject only to three instead of six pounds, it must be less liable to explode or burst at that than at an increased expansive force. I should further say, that every man that is called to work a condensing steam-engine, knows, that when his steam is at three pounds and a half, it performs a greater quantity of labour than at any other time; for if you increase it you throw a vast labour on the air-pump and the condenser, and retard the engine: therefore, a man has no inducement to increase the expansive force of the steam, he knowing that no useful end can be obtained by so doing, but giving himself additional labour and consuming more fuel, and performing less work. I should also wish to state, that I yesterday made a sketch of what appeared to me to be a proper and efficient boiler for a steam-boat,

without

without reference to the character of the engine at all, whether it was a condensing or high pressure engine. All boilers on board steam-boats should have the fire in the interior of the boiler; because it is of very little importance, when you are upon the subject of safety, whether the passengers are to be endangered by an explosion, or whether the vessel is to be weakened in its timbers or essential securities by the improper application of the fire to the boiler: therefore, I invariably recommend, that the fire should be contained in the interior of the boiler, and that there should be an additional safety-valve, which should be solely subject to the superintendence of the proprietor, and that the manager of the machine should have no possible access to it.

That you mean to apply, whether high pressure or low pressure boilers are used?—Both; because I am quite aware, that if a boiler in a steam-boat is to have the fire to operate upon it externally, although you may not explode the engine, you may so far destroy the vessel that carries the engine by burning its timbers, without the knowledge of the individuals to whose care the boat is intrusted, as to be highly injurious and mischievous to the safety of the passengers. I should certainly recommend a wrought-metal boiler in preference to a cast-iron boiler; and the reason is clear, that the operation of casting, however skilfully managed, is always an uncertain process. An occurrence took place a few days ago, which very much staggered me; I had a large press of cast iron, which it was necessary to break up, and in the interior of a bar which was probably eight inches by twelve, there was a cavity in the centre of four inches diameter, with no external communication.

Do you think that a safety-valve may not be so constructed, as that its operation shall not be impeded in any degree by the engineer to whose care the vessel is committed, and yet with a tolerable certainty of its operating to all its proper intents and purposes?—If an additional safety-valve was applied to a boiler, and that safety-valve placed beyond the power of being interfered with by any person but the proprietor, then the boiler would be secure from explosion, if the safety-valve should be judiciously loaded; but if that safety-valve was even placed beyond the reach of the operator, and at the same time injudiciously loaded, a calamity might take place the same as if no such security existed.

Allowing that under all possible circumstances a condensing engine should be the most safe, what is your opinion as to the sufficient safety of a high pressure engine, of which the boiler and safety-valves should be constructed in the manner which you have just now described?—I should consider a high pressure engine,

gine, under such circumstances using the expansive force to forty pounds to the inch, and not beyond forty pounds, would be a safe and efficient engine.

Under all the circumstances which at present exist in the manufacture and management of a condensing and high pressure engine, with a view to the safety of passengers in a steam-boat, which of them would you recommend?—Under all the circumstances of the case, I should most decidedly recommend a condensing engine, a condensing engine with [illegible] boiler; because when cast iron becomes subject to [illegible] expansion and contraction, the constant repetition of [illegible] in a very great degree impairs the strength of the boiler.

That mischief would not be incidental to a wrought-iron boiler?—Certainly not. I should venture to say, that all engines in steam-boats should be subject to regulation and inspection by competent persons;—a steam-boat must have a register, and before such register should be granted, the engine should be inspected, to see whether it is of a character to deserve its being considered safe.

What is your opinion as to the expediency of adding a mercurial gauge?—By no means do I consider it an efficient and convenient apparatus on board a boat; it would be constantly in the way, and it would require a great column of mercury to make it safe; and that such a quantity may be liable to do mischief if blown out.

Has it ever happened to you, to form any calculation of the proportion which a mercurial gauge ought to bear to the diameter of a boiler?—I have not; but it will depend upon the expansive force to which the boiler is to be brought up, as well as to the capacity of the boiler; because, if you were to put a mercurial gauge to give merely the pressure on the boiler, that would not be adequate to carry off the quantity of steam that may be generated in a mischievous way.

What is your opinion as to the comparative consumption of coals in condensing and high pressure engines, with respect to the work produced?—I am quite satisfied, that taking for granted that both engines were judiciously formed, the one would take as much fuel as the other, there would be no material saving, if any; but if you associate the two principles together, as in the case of Woolf's engine, there will be a considerable saving; unite the high pressure with a condensing engine, and there is a great saving, but in their abstract characters there is none.

Mr. John Braithwaite's *Evidence.*

What is your profession and place of abode?—I reside in New Road, Fitzroy-square, and am an engine-maker and engineer.

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The Committee being desired to report upon the safety of steam-boats, and upon the safety only, they will be much obliged to you to communicate what you know upon the subject?—Respecting high pressure steam, which I shall confine myself to at this moment, I will engage to make a boiler, or direct one to be made, which I will defy any engineer or other person to blow up or burst; and I have lately erected five boilers; and I am ready to prove to any gentleman, and even to any engineer, that they cannot destroy them.

Upon what principle were those boilers constructed?—Those boilers that I have fitted up, with the different apparatus for making them secure, were made of wrought iron; but I do not mean to say cast-iron boilers cannot be made secure. I recommended to Mr. Martineau, for whom I erected them, that as there had been an accident in his neighbourhood, he ought to have a boiler to bear three times the pressure he meant to put upon it; and if it did bear that pressure, and they applied two safety-valves with a mercurial steam-gauge, properly weighted and adjusted (one of those safety-valves should be at the will of the person about the boiler, and the other no man should be able to get at), it would be impossible to explode a boiler of that description. I saw the boiler after it was exploded at Wellclose-square, and also conversed with one of the men that was saved, who told me, that he had carried an additional weight to put on the safety-valve just before it exploded, that the mercurial gauge there was plugged up, so that it was useless; besides which, instead of the safety-valve being weighted equal to forty-five pounds, they added a double weight which increased it to ninety pounds weight upon an inch, and the boiler was very improperly made. I conceive that a steam-engine boiler, constructed as it ought to be constructed (I do not mean to say if you put a boiler into the hands of men not acquainted with it, without the proper safety-valves, there may not be danger)—but if properly constructed there is no danger.

Would you not recommend on board steam-boats, wrought-metal boilers to be used in preference to cast?—Certainly; I have made some discoveries myself in the boilers I have put up, which makes them perfectly safe.

Do you know any thing respecting the comparative consumption of coals in high and low pressure engines?—Not from my own actual experience, only from what gentlemen have told me where I have done business.

Mr. John Hall's *Evidence.*

Where do you live?—At Dartford.

What are you by profession?—An engineer and millwright.

Have

Have you given any attention to the construction of engines for steam-boats?—I never have; I have made steam-engines, but not for steam-boats.

The object of this Committee being to inquire into the construction of engines for steam-boats for the safety of passengers, have you any thing to communicate to the Committee on that subject?—I have only to observe, that I make them in cast iron, and I have proved them by an hydraulic press made for the purpose, and have gone as high as 250 pounds to an inch, and that I considered enough; nothing happened; and I mean the next time to try what they will bear, and I have no doubt they will bear from 700 to 1000 pounds to an inch, for I believe they can be made now stronger than wrought-iron boilers; wrought-iron boilers being riveted together, cannot be so strong as those cast in a solid mass.

May not there be some imperfection in cast iron, which may not be discoverable without an accident happening?—It is scarcely possible, if it undergoes the trial I speak of by pressure before it is put to work.

May not that trial to which it may be exposed, though no accident happens immediately from the trial, be injurious to the boiler itself?—If it is made so as to be strong enough to stand the pressure of 500 pounds upon the inch when it only wants fifty, I suppose that proves it to be quite out of danger.

Are you aware that there is a difference between trial made by water-pressure at a certain temperature, and the exposure of cast iron to the action of fire repeatedly, by which the metal is heated to a very high degree, and consequently expanded and then cooled again down to a temperature very far indeed below that which it was before exposed to?—I have seen the effect of that; a boiler I have made has been composed of three tubes, one a large one and two smaller ones below; those lower tubes which are exposed most to the fire have cracked generally by cooling after the engine has done working; I have known that in three or four instances; perhaps, in an hour after the engine has done working, the tubes below have cracked and the other not.

Are you not aware that the tubes which were so cracked by the application of fire, might have stood the water-pressure of which you before spoke, to almost any conceivable amount?—Yes, I suppose they would.

In case of explosion,—which would produce the greatest mischief, that of a cast or of a wrought-iron boiler?—I suppose the greatest danger would be in the wrought-iron boiler.

For what reason?—Because the cast iron uniformly cracks at the bottom underneath the large part of the boiler; the bottom

tubes have cracked on the under side, so that the water went away.

Did you never hear of any instance where a cast iron boiler has exploded in another way?—I have heard of the late misfortune at Norwich, and that has been sufficiently accounted for to me, by its being made so very improperly.

Have not you heard of other instances of cast-iron boilers exploding?—I believe only one.

Is not a cast-iron boiler liable to be exploded in fragments? —I should think it would never happen, if it was made as cast-iron boilers ought to be made; I suppose we might make a cast-iron boiler that would explode, and go to pieces in that way, if it was done on purpose.

Have you any other suggestions to make to the Committee? —As to safety-valves, they may be made as safe as can be conceived of, because they will let the steam escape when it is of an improper height, and these engines I am making will save in fuel very materially; they are on Woolf's principle; they will save two-fifths of the fuel.

Is it not easy to adjust a safety-valve to a boiler, which shall not be accessible to the engineer directing the machinery, which shall sufficiently protect the boiler from mischief?—Yes, it is quite practicable.

And so to adjust it that it will always act?—Once adjusted it will always act, and always be to be depended upon.

Then you would recommend, in any boiler, such a safety-valve to be employed?—Certainly.

Besides another under the direction of the man who works the engine?—Yes.

Mr. Alexander Tilloch's *Evidence.*

Will you state where you reside?—At Islington.

And what is your profession?—I am editor of the Philosophical Magazine, and sometimes I am called on to act as an engineer; and I am editor and proprietor of the Star newspaper.

Will you be so good, as you know the object for which we are met, with regard to the safety of persons in steam-boats, to mention what suggestions you have to make to the Committee on the subject?—My opinion is, that attending to what should be attended to in every steam-engine, and employing proper engineers, a steam-engine would be perfectly safe, whether with high pressure or low pressure. The boilers ought always to be furnished with safety-valves; and if they suspect the possibility of having a stupid man, one of the valves should be covered and out of his reach with a box over it, but perforated so that you may see when the steam operates on it. A mercurial valve is also

also very good; that is an inverted siphon, with a column of mercury proportioned to the purposes for which it is to be employed.

Do you apprehend much danger to arise, in case of explosion, from that mercury if it was employed?—No, because the tube is always perpendicular, and if the mercury shoots out, it goes away and falls down in rain; I am of opinion, a boiler may be made safe either of wrought or cast iron, but for great strain I would prefer cast iron, contrary to the opinion of many people, and the reason I would prefer it is the same for which it is preferred in making cannon. It is not possible to get thick plates of wrought iron perfect throughout, and you trust at last to rivets in joining them, but cast-iron boilers can be made of any strength you please; instead of having a boiler that will stand sixty, it may be made to stand six hundred, of either wrought or cast iron. Another reason why I would prefer cast iron is, that the sheet iron corrodes much quicker and destroys by oxidation, so that a boiler may be safe when first set up and stand its proof, but very soon become unserviceable, or at least comparatively so. Boilers should always be cylindrical tubes, and for an obvious reason, capacity should be got by length and number rather than by diameter. There is no more danger to be apprehended from steam as to bursting, than from the employment of condensed air, only that the water may scald; but as to the danger of the fragments being scattered about, it is the same with air as with steam, and yet all the engineers constantly employ cast-iron receivers, condensers, or air-vessels where pressure is wanted.

Is not cast iron liable to suffer some material injury from the contraction and expansion by heat and subsequent cooling?—Whether a boiler be made of wrought or of cast iron the metal expands and contracts, and expansion or contraction is more or less injurious in proportion as it is often repeated, but it does not prejudice a boiler made of cast more than one made of wrought iron.

Is not it more injurious to cast than wrought-iron boilers?—No, I do not think it is.

In case of accident by explosion in a cast and wrought-iron boiler, which, in your opinion, would be attended with the greatest mischief to the persons about it?—If an actual explosion takes place, I should think from the cast iron; but I conceive that a properly constructed cast-iron boiler would be stronger, and therefore would not explode so soon. A boiler should be proved with cold water, if it is to be applied to high pressure.

Are you not aware that cast iron, notwithstanding the greatest possible attention of the founder, is liable to cavities in the interior

terior substance of the metal, which renders it uncertain when exposed to great degrees of heat?—There may be cavities in cast iron, but a boiler being proved to a strain beyond that it is to be exposed to by heat, the safety of the boiler is secured; for the temperature never can be at that point which will endanger a fracture from that circumstance.

Do you mean by that answer, to say that the rarefaction of the air in that cavity may not be so great by the heat as to occasion its bursting?—It never can, because the air that produced that cavity was at a white heat at the time the iron closed upon it, and it never can be brought to such a heat in working a boiler;—my opinion is, that by a very high proof at the commencement, and attention to it, you may always have a safe boiler of cast iron.

[To be continued.]

XXVII. *Memoir of* Abraham Gottlob Werner, *late Professor of Mineralogy at Frieberg**.

Abraham Gottlob Werner was born on the 25th of September 1750. His father, who was inspector of an iron-work at Wehrau, on the Queiss, in Upper Lusatia, intended him from his early youth for a similar vocation. He first went to school at Bunzlau, where he received however but very scanty instruction. In order fully to qualify himself for his intended profession, he went first for some years to the Mineralogical Academy at Frieberg, and then to the University of Leipsig, where he applied himself to the study of natural history more than to that of jurisprudence; and in respect to the former used to boast in later years of his intimacy with two distinguished naturalists of Leipsig, Mr. John Charles Gehler, and his brother John Samuel Traugott Gehler. Even while at the University he employed himself on the doctrine of the external characteristics of fossils, in which a singular quickness of perception was of great use to him; and published there, in the year 1774, the well-known work (on the external characteristics of fossils) which is still considered as the basis of his whole oryktognosis, but of which he could never be induced to print a new and enlarged edition, because he feared disputes, and had not in fact concluded his researches. Soon after he was invited to Frieberg, to have the care of the cabinet of natural history there, and to read lectures upon it. Here his mind, which was early exercised in observation and classification, found the most welcome materials. Here, daily extending the bounds of his science, and

* From The Literary Gazette.

supporting

supporting its foundation by the surest external distinctive marks, he formed that system which, afterwards embracing also the geognosis which was peculiarly his own, and forming an intimate connexion with all branches of the art of mining, gradually conquered all opposition, and raised its inventor to the rank of the creator of a new mineralogy, which might be supported and extended, but not rendered useless by the crystallographic theory of Haüy, and the chemical theory of Vauquelin and others. His peculiar talent for observation was animated by the most lively fancy, assisted by the most extensive reading in every branch of knowledge connected with his own, and excited by daily intercourse with ingenious travellers and foreigners, who chiefly visited Frieberg on Werner's account. (We may instance only the Englishman Hawkins.) The classification in genera and species, and for the most part ingenious appellations of minerals down to the newest *egron*, is peculiarly his. "Werner," says Leonhard, in his eloquent lecture on the state of mineralogy, "was for the doctrine of the recognition of simple fossils, embracing with uncommon ingenuity all the experience of his age, what Winckelmann had been to the arts. What, before him, were all the endeavours of Wallerius and Linnæus!" How soon was he obliged to give up Cronstedt, who is no where satisfactory! Only too scrupulous, conscientiousness prevented him from publishing the oryktognostical tables, which have been finished, and quite ready for the press these four years. The attempt of the ingenious Berzelius, of Stockholm, at classification by discovering the laws of combination of the elements, did not indeed shake his belief in the method of recognition by means of the external characteristics; yet he at last thought that a mutual conciliation was possible, and reserved the first analysis of the latest writings of Berzelius, for the next winter. Block's work was known to him. He approved of his ingenious scholar's (G. H. Schubert's) essays (*Ausgleichungsversuche*). In the geognosis, first systematically deduced by him from the rough mass, crystalline structure, and the chemical relations of the contents, may be called in, together with the ties of external affinity; but the method created by Werner is the only satisfactory one, however much may yet be wanting to it, to become a complete system of the earth. His predecessor Charpentier's doubts respecting Werner's theory have never been able to shake it. His idea of formations, one of the most fruitful of consequences, and the most ingenious, in Werner's geognosis, has been admirably developed by his scholar Steffens in Breslau; and his formation of the floetz mountains of Thuringen, well supported by the excellent Von Freiesleben, in the theory of the copper-slate mountain (*Kupferschiefergebirge*). Werner sustained an obstinate,

tinate, but for that reason the more honourable contest with the volcanists. Now, no well-informed person will consider the basalt and other flœtz mountains as of volcanic origin. Werner's theory of the older and newer formation of mountains, by the waters, stands immoveable; and a satisfactory link between them is afforded in the mountains of the interval of transition. Even the new chemical discoveries of the *kalimetals* may be made to accord with it. Another science, Mining, on which Werner used also to lecture, was rendered extremely clear to the attentive scholar, by his luminous explanation and by the reduction of the most complicated machinery to the most simple propositions, at the same time drawing all the figures on his table. Indefatigable application, insatiable thirst of knowledge, enriched his retentive memory with every thing that history and philology, in the most extensive sense, can offer to the attentive inquirer. No science was foreign to him. All served as a basis to his studies, which were constantly directed to natural philosophy, and the knowledge of the earth and its inhabitants. He always advanced before his age, and often *knew* what others only *presumed*. After 1779 and 1780, when he first lectured on oryktognosis and geognosis, at Frieberg, he was heard with gratitude by scholars from all parts of Europe. Never contented with what was discovered, always seeking something new, he rather formed scholars who wrote than wrote himself. But many MSS. almost wholly ready for the press are included in his fine library, collection of coins and MSS. bequeathed on the day of his death to the Mineralogical Academy, for 5000 crowns. In his lectures he had only heads of the subject before him. In lecturing he used to abandon himself, as he was accustomed to say, to the inspiration of his mineralogical muse; and when his spirit hovered over the waters and the strata, he often became animated with lofty enthusiasm. But he caused his lectures to be written out by approved scholars; and by revising himself what they had thus written after him, made it, properly speaking, a MS. A great many parts of his lectures have been made public by others, among which may be reckoned what Andic, at Brunn in Moravia, has published in the valuable journal Hesperus. But nothing bears the confirmation of the seal of the master. What is particularly desirable is the publication of his manuscript on Mineralogical Geography (which he only once drew up for a particular lecture), and upon the Literature of Mineralogy, in which he solved the difficulties of the ancient classic mineralogy, and gave incomparable illustrations of Pliny's Natural History. He was like a father to all his scholars, to whom he was a model not only as a man of science, but as a moral character. Having filled, from the year 1792, a high situation in the Council

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of the Mines, he had a great share in the direction both of the Mineralogical Academy and of the administration in general. Two things must be mentioned here with particular honour—the works begun in 1786, to furnish a great part of the deeper mines with water, in order to get water for driving the wheels. This astonishing aqueduct, particularly the artificial canal of Doerrenthal, with its subterraneous bricked channels, already extending above a league, are in the main due to him, though Scheuchler made the plan, and Lampe the calculations. By the continued support of the ever active king of Saxony, this great work still proceeds in the most prosperous manner. The Amalgamation works, twice built by the excellent Charpentier, chief of the Council of the Mines, (the first building was maliciously burnt down,) and for ever secured by most ingenious fire-engines from similar accidents, are indeed unique:—a miracle to all who behold them, and a jewel in the crown of the Saxon art of mining, and of the unostentatious energy with which the sovereign of Saxony caused the most expensive undertakings to be executed in silence. Less known and visited by foreigners, though on it depends the continuation of the mining in Saxony, is this undertaking of canals and aqueducts, which has already cost above half a million of crowns, and on which more than a thousand men are employed. The mineralogical survey and description of all Saxony, divided into districts, which has been prosecuted for these twenty years, under scholars of Werner, and includes the forest of Thuringen, and even a part of the Harz, uniting too with the mountains on the frontiers of Bohemia and Silesia, will one day give our country a mineralogical map, which for exactness and extent surpasses what any other country can produce. This too was Werner's work, and was constantly directed by him in the most attentive manner. In his visits to Prague and Vienna, he found means to interest the Austrian government in these mineralogical surveys; and it is to be hoped that the enlightened Bavarian government, as well as the direction of the mines in the Prussian monarchy under Werner's grateful scholars in Berlin and Silesia, will readily contribute to support and complete the great work which Werner so happily set on foot. In England and Scotland excellent mineralogical maps of single counties have lately been published according to Werner's ideas. His cabinet of minerals, unrivalled in completeness and scientific arrangement, and consisting of above 100,000 specimens, has become, in consideration of a life annuity, the amount of which devolves to the Institution itself, the property of the Frieberg Mineralogical Academy. Werner's favourite pupil Koehler is appointed inspector of it. Werner had received from England an offer of 50,000 crowns for it. He sold it to his country for

40,000,

40,000, of which he reserved the interest of 33,000 as an annuity; but made the condition, that after his own death, and that of his only sister, who is without children, the interest should continue to be annually paid to the Mineralogical Academy; so that this, his only daughter, as it may be called, obtains an additional annual income of 1600 crowns.

Werner's literary studies, like his mind, embraced every branch of science. Every thing excited his thirst of knowledge, and thus it often happened that he dedicated all his attention to researches which seemed to lie entirely out of his sphere. His inquiries into the direction of the mountains of the first and second formation, led him to the seat and the migrations of the aboriginal tribes and their branches. To this were soon joined inquiries into the original languages and radical syllables, which he prosecuted with the greatest acuteness, and reduced into tables. Soon arose an universal glossary of all the radical syllables and characteristic sounds, in all the languages with which he was acquainted; which he studied with ardour, and to complete his knowledge of which, he purchased the most expensive works; thus he gave sixty crowns for Hickes' Thesaurus, and but lately eighty crowns for Walton's great Polyglot. His antiquarian researches into the mineralogy of the ancients made him a passionate friend of archæology, and the most costly works on that subject were purchased by him. One branch of archæology, the numismatology of the ancients, had become so favourite a pursuit with him during the last eight years of his life, that he purchased entire collections of medals, and in a short time was in possession of above 6000 ancient Greek and Roman coins: this enabled him to make interesting researches into the different mixtures of the metals, and on the arts of adulteration; and in order to make all more clear, he arranged entire series of false coins. An unedited silver coin of his collection, which he gave to the great connoisseur Catauro, in Milan, is still the subject of a numismatic controversy between the Vienna and Italian connoisseurs. The examination, which was to be printed, was intended to be dedicated to Werner. The practice which he had had in studying the direction of the mountains and the surface of the earth, made him an excellent judge of ground, and inspired him with a great fondness for military tactics. He studied the art of war with great diligence, read the accounts given by masters in this branch, and acquired a fine collection of military books. Officers of the engineers and general staff were surprised to hear him speak of the mistakes committed by the allies from want of due knowledge of the ground, in their attack upon Dresden in August 1813, where he happened to be present. His name was mentioned at the head quarters of the allied sovereigns

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at Frankfort, and he was invited to repair thither; but his inflexible attachment to his king made him decline the invitation. Medicine also attracted his attention, at first as lying in the circle of the sciences connected with natural history, but afterwards in the latter years of his life, that he might be enabled to judge of the bodily sufferings of himself and others; so that medical books were his favourite reading, and conversation on medical subjects what he preferred to every other. Ever ready to afford assistance, he was happy, when he visited a sick friend, to be able to give medical advice, and also to judge of his own situation which he often thought precarious. The danger of such an inclination, which can never lead to any thing further than empiricism, is evident. His best friends, among whom we may reckon the veteran of the healing art, the venerable Dr. Kapp, at Dresden, sometimes reproved him for this; but it remained his favourite hobby-horse. He had made a very witty table of diseases according to the stages of human life, from infancy to old age: he was a sworn enemy to vinegar and all kinds of milk diet, but a determined beef-eater. In other respects he lived very temperately, drank but little wine, and was especially and anxiously careful about warm clothing and warm rooms. He first visited Carlsbad, when a boy of only fourteen years of age, and had since been there forty-one times. Here, even in the latest part of the autumn, he always acquired new strength. Had not imperious circumstances hindered him this time from visiting sooner the salutary fountain, which had become absolutely necessary to him, he would perhaps have still lived. He was fond of travelling, and spoke with emotion and pleasure of his visit to Paris in 1802, where he was received with the greatest respect. Though not indifferent to external distinctions, to the diplomas of foreign academies and learned societies, he never sought or asked for them, and in conversation never attached any value to them. However, he was justly proud of being a member of the Institute of France, and of the Wernerian Society in England. Even on his death-bed he learnt with joy from his former pupil and faithful friend the Professor of Natural History at Edinburgh (Jamieson), that not only several mineralogical societies flourished in Great Britain, but that professorships of mineralogy on Werner's principles were founded at Oxford, Cambridge, London, Glasgow, Cork, Dublin, and Belfast. At his suggestion a union of friends of natural philosophy and mineralogy was formed last winter in Dresden, where Werner himself presided. He was in the best sense of the expression a citizen of the world. Every newspaper that he read, excited in him a pious wish for the happiness of mankind, for truth and justice. In the last days of his life, his eye was most frequently directed

directed to the Brasils, where the excellent Oranjo was his friend, and many Germans now employed there his scholars. In his thoughts he followed every traveller, and put questions to him, in his own mind, such as Michaelis once wrote for Niebuhr and Forskael. His house was the constant rendezvous of curious travellers, from all countries and of all ranks; and he showed to them all, with uncommon patience and attention, his museum, and especially his collection of precious stones, which excites surprise by the value and variety of the specimens. He did not, however, like writing letters, because he preferred personal intercourse to every thing, and dreaded a loss of time. This disinterested participation, in whatever promoted in any country the interests of knowledge and humanity, did not hinder him from being the most faithful son of his own country, the most loyal reverer of his king. He refused every invitation from abroad, (and he received at an early period several very brilliant and enticing ones,) and was for many years contented with a very moderate salary, supporting himself by private lectures. He made presents to all the academies and public schools of Saxony, and endeavoured by this means every where to excite a predilection for natural philosophy. Those who were most intimately connected with him, enjoyed his tenderest interest and care.—" In his house," said Boettiger, in his farewell address on the eminence of Gorbitz, " company daily assembled for his advice; and the same hand with which he felt the pulse of nature, raised and supported every unfortunate. His simple manners, his cordial cheerfulness, and his social playfulness, made him the favourite of his fellow-citizens. When Werner entered, every countenance brightened; the women, too, loved the company of a man who, without insipid compliments, always had something delicate and entertaining to say to them. In his earlier years his feeling heart would doubtless have made him highly susceptible of enjoying the sweets of domestic life; but he did not find what he sought. In later years he renounced the idea of them, out of love to science, and was fully indemnified by the cordial attachment of his pupils and friends. Penetrated with that true devotion which worships God in spirit and in truth, he often preached to his pupils the purest morality, which he confirmed by his own example; and even in his lectures often rose with genuine enthusiasm from the miracles of nature to their Divine Author.—Such was the man of whom his contemporaries and his country will be always proud; a man equally distinguished by his rare learning, and by his goodness of heart and unspotted character. How just is the grief caused by such a loss! His fairest monument is the gratitude of his pupils, who are spread over all the countries of the world. But his doctrines and his

life

life will not fail of public acknowledgement and praise. This tribute will be given him from France, England, and Italy. Neither must the tongue of his pupils in Germany be mute. May Von Leonhard dedicate to him his second lecture in the Academy at Munich! May Steffens, Ullmann, Hausmann, Mohs, Moll, Linke, and Weiss, and above all the feeling Schubert, speak of him! May Gilbert, who defended him against the violent Chenevix, erect a memorial to him in his Annals!—Nor can we doubt but some monument of marble or bronze will be raised to his memory, to which British gratitude and generosity will gladly subscribe, and Frieberg afford a suitable situation to be inclosed for the purpose. For the present we hope that Bôhme, or Buchhorn, will engrave the fine portrait of him, by G. Von Kugelchen, in Dresden, which was intended for his museum, for the satisfaction of his numerous scholars and friends. His most glorious monument, however, will always be the Mineralogical Academy, preserved in uninterrupted activity by his worthy scholars; that academy which he himself sometimes called his beloved daughter, and richly endowed; those who go thither on a pilgrimage, those who there receive instruction, will pay continued homage to the manes of WERNER.

XXVIII. *Preface to "The Natural History of the Mineral Kingdom. By* JOHN WILLIAMS, *Mineral Surveyor, F.S.S.A."**

GREAT BRITAIN has long ago been called a fortunate island; and it must be acknowledged that the appellation is as proper to Britain as to any other island or country in the world. The soil of this island is adapted to produce excellent grain and fruits. Her downs and verdant hills are covered with store of the best of sheep, which yield excellent fleeces for our manufactures, as well as food for our tables. Numerous herds of beeves are fed upon her mountains and in her meadows, and her seas and rivers abound in the most delicious fish. The climate of this island is mild and healthy; her mountains breathe the purest air, and abound in the sweetest springs, and her valleys are washed and fertilized by pure and limpid streams.

This fortunate island is placed almost in the centre of the habitable world, with free and ready egress to the Mediterranean, the Baltic, the East and West Indies, and all other seas to the south and north;—the most convenient situation for extensive

* See last Number of Phil. Mag. art. 17.

commerce,

commerce, which is greatly facilitated by the safety and prodigious extent of her sea-coasts, the depth and numbers of her rivers, and the depth and numbers of her bays and sinuses all round the island.—Her forests produce the hardiest oaks for ship-building, and her sea-ports the best and hardiest sailors, who are in a manner bred upon the water; and no island in the world ever yet arrived at such commercial eminence, and, in consequence, at such a height of power, wealth, and grandeur.

But it is not all the external productions of this island put together, favoured as she is by the goodness of her soil and situation, and assisted by the excellence of her constitution, and the utmost exertion of the genius of her sons, that ever was, or ever will be able to raise her to such a height of power and wealth, or to such commercial and political consequence in the world. The soil of some other countries is as good as that of Britain. The island of Sicily produces as excellent grain and better fruits, and some parts of Spain as good, if not better fleeces. But Britain has other valuable sources of commerce and wealth. The materials of many of the various and extensive manufactures of the island, are derived from the bowels of the earth, from her plentiful mines and coal-works.

This is the source of the materials of our most numerous and extensive manufactures, and of the utensils of them all; and it is our manufactures that fill and extend the channels of commerce, and bring home our wealth from afar.

This island is a nursery of arts, as well as of manufactures and commerce.

It is a curious and entertaining amusement to reflect upon the connexion and dependence of the arts upon one another, and upon the improvements and advances of society in a polished commercial country. A man of genius and of judgement, equal to the task, with a stock of information and scope of thought like Raynal, who would write a book to show us the progress and improvements of the useful arts, the æra of remarkable discoveries and their effects, and the influence which the improvements of the useful arts have upon the commercial and political state of the nation, and of the world in general, would deserve the thanks of his country for the extensive information, useful instruction, and national entertainment which his book would afford.

Perhaps it would then appear, that the great quantity and variety of metal which this island produces has more influence upon the commerce, wealth, and power of the nation than we are accustomed to imagine at present. But as I have neither abilities nor materials for such inquiries, I will leave them to be investigated

gated by such as are equal to the task. This much, however, appears very obvious to me, that great numbers are profitably employed in our mines, and in conveying the metals out of the nation; that the value of these metals, whether raw or manufactured, is all clear gain to the nation; that still greater numbers are employed to work upon the metals for all useful and ornamental commodities, and for all utensils, trades, and arts.

What is done without the metals? Look into the kitchens and buffets of the great and wealthy: what profusion! And yet all for use. When we pass through Cheapside in London, one might imagine that all the metal of the world was furbished up and arranged there for his inspection; and yet it is in some proportion equally plentiful every where. The utility of the metals is analogous to their abundance. The mathematical-instrument-maker does but little without them, and much is used by the blacksmith, whitesmith, coppersmith, pewterer, tin-plate worker, coachmaker, cabinet-maker, clockmaker, silversmith, engraver, printer, &c. The quantities used by the various sorts of founders, and the plumbers, are immense.

But if you would wish to have a full and comprehensive view of the profusion and great utility of the metals, step into the workshops and warehouses of Birmingham. How many thousands are there at work! What amazing quantities of wrought goods are stored there ready for exportation and home consumption! There you will see them busied in making all that is worn of metal by the lady and her maid, the clown and the beau, the horse and his rider, both for ornament and real use; and their warehouses contain enough for half the world, which must pass through the channels of commerce. In short, the plenty and variety of our own metals, and the plenty and excellent quality of our coals, enable us to manufacture and export more and greater variety of metallic goods than any other nation whatever.

From this imperfect sketch of the profusion and extensive use of our metals, I would infer the great importance of the British mines to the commerce, wealth, and grandeur of the nation; and I would likewise infer the importance of improvements in the natural history of the mineral kingdom in such a country, especially at this period.

Mineralogy is now become a fashionable study in most countries of Europe, and many useful and entertaining discourses have been made of late years. But the present vogue and reputation of this branch of knowledge is nothing in comparison of its great utility. There cannot be a more interesting study for a Briton; for while we have extensive mines and collieries, and while the production

production of them can be obtained at a moderate expense, we shall be considerable as a manufacturing and a commercial people.

It is a particular loss to the increase of knowledge in the natural history of the mineral kingdom, that this branch of science is neglected in our public schools. Mineralogy is taught in the universities abroad. I believe, that what may be called fossilogy, or the arrangement and description of mineral fossils, is taught in some of our public schools; but their instructions are founded upon small detached samples, the collections of the cabinet, which leave the country gentleman and the young miner as much in the dark as before, with respect to the knowledge of Nature and of real mineral appearances, which are the true sources of useful knowledge in these matters; and this species of knowledge is of great importance.

No country in the world depends so much upon the productions of the mineral kingdom, for the means of comfortable accommodation, wealth, and power, as the island of Britain.

Coal is now become of such immense consequence to our cities and populous counties, to our forges and other manufactures, that it was impossible for us to have arrived at such commercial eminence, and it is as much impossible for us to support our present flourishing state of society without it; and we are equally indebted to the other parts of the mineral kingdom for many of the staple commodities, which are so widely diffused in the numerous channels of our extensive commerce.

When we consider that many thousands, I may say millions, of industrious hands are employed one way or other about the produce of the mineral kingdom in this island, we are convinced of the importance of the increase of knowledge in mineralogy, and of the advantage that would accrue to the nation from the institution of a class for teaching this science at our public schools.

It may be said, that the necessary aids for such an institution are wanting in this island;—there has not yet appeared any genuine natural history of the mineral kingdom, founded on such sound principles of philosophy, as would enable a teacher to lay the foundation of, and to complete a continued course of instructions in the science of mineralogy. There are not, that I know of, many valuable books upon the subject in our language, excepting such as treat of chemistry or metallurgy, and such as arrange and describe fossil bodies, as they are found in the cabinets of the curious,—almost all the rest is nothing but wild theory and system, built upon fanciful notions and opinions, the fruits of the closet, which have no foundation in the truth of facts, as they appear in natural history; and therefore such books can be

be of no use but to abuse, to multiply diversity of opinions, and to increase ignorance of the real knowledge of nature.

It is this consideration which induces me to give to the public a work the fruit of more than forty years experience and observation, to which great opportunities and a mind ardent in researches of this nature prompted me. How I have executed my plan, the public shall judge; but I flatter myself, from the great number of facts I have ascertained, and from the many discoveries I have made, that my observations may be productive of real use to mankind, by exciting the pursuit of, and giving a proper direction to the study of this science, with more pleasure, ease, and proficiency than hitherto.

The knowledge of truth in every branch of science is pleasant and profitable; and it is generally acknowledged, that natural history is the most pleasant and profitable of all human studies and researches; and of all the parts of natural history, the mineral kingdom is the most magnificent and august, provided that we study nature herself.

There is a noble air of grandeur and magnificence in the sections of lofty piles of strata, in huge rugged rocks, and hanging precipices, in profound caverns, and high and extensive cliffs of the sea, not to be found in order objects around us.

These scenes astonish and captivate the mind at first sight; and the better we are acquainted with them, the more we are enraptured with the view of the wonderful and endless variety which we discover in these scenes of nature; and habitual application to these researches assimilates the mind by degrees to the greatness of the subject.

Discoveries of truth and attainments of knowledge in these researches have the happiest effect on the human mind. In pursuing these studies successfully, the mind is elevated, the understanding is enlarged and filled with great ideas, and all the powers of the soul are exalted and pleased at being able to comprehend somewhat of these great works of God.

In short, I conclude that there are no human studies so amusing, so entertaining, and delightful as these, when the student delights in the sequestered scenes of nature. There is such a dignity and variety in every part of this subject, that it is impossible for a person of any genius and taste to be cloyed with these pursuits.

Who can possibly weary of endless change, and all either astonishingly great, or fantastically grotesque, or beautifully regular; and I know well, that the more we improve in the knowledge of these natural scenes, the more we delight in them; and therefore, without being a prophet, I will venture to predict, that whenever young gentlemen of genius and attention take pleasure in

in these researches, it may be then proclaimed, that the darkness is past, and that the glorious light of science is rising upon the mineral horizon.

Great and rapid progress will then be made in this branch of natural knowledge, and the mineral kingdom will soon be understood as well as the animal and vegetable kingdoms. But the importance of these studies should be preferred to the pleasure of them in this mining, manufacturing, and commercial country, where it may be supposed there are but few landed estates that do not contain some mine or mineral fossil or other, which may contribute to the public good, and to private emolument; and, therefore, I wish to excite a lively sense of the importance of increasing mineral knowledge.

In such a country as this, young gentlemen of landed property should be initiated in the principles of mineralogy, and such youth as aim at professional abilities in mineral lines of business, should have it in their power to lay an early foundation of knowledge in this branch of natural history, which is the best way to arrive at eminence in the stations they are intended to fill.

I have, in the following sheets, contributed a small moiety towards the acquisition of knowledge in mineral science.

I have treated pretty fully of the natural history of the strata of coal, and of such other strata as are found to accompany coal; I have treated fully and distinctly of the appearances, indications, and symptoms of coal; and I have been very careful to distinguish the real and certain appearances from such as are either false or doubtful.

In this part of my subject I have taken due pains to investigate and explain every thing that I thought would throw light upon, and communicate useful information, relating to a subject of so much importance to society; and I am persuaded that my treatise upon coal will be of use to landed gentlemen, towards facilitating the progress of youth in the knowledge of this branch of natural history, and as an index for the young coal-master.

The second subject treated of in this work is the Natural History of Mineral Veins, and of the other beds and repositories of the precious and useful metals. I did not at first intend to publish my treatise concerning metallic mines at this time, because it is not completed; but when the first part of my work was put in the press, I reflected that this second part contains a number of particulars which may be useful to landed gentlemen and young miners; and as it is very uncertain whether I shall proceed any further in these mineral essays than the two volumes now published, I thought it was better to offer this in its present imperfect state, than to suppress it altogether.

The history and description of mineral veins is perhaps more full

full and explicit than can be found any where else. The precepts upon shoading and hushing are the result of much practice: the observations and instructions concerning the appearances and symptoms of mines will give satisfaction, and be a sure guide to all such as have occasion to consult them; and the local examples of the appearances of some valuable mines may, in the course of time, be of great use to society.

Such historical facts have always been considered as valuable communications. In short, all that is advanced in this imperfect fragment is the fruit of my own observation and experience; and, therefore, it should be of some value, such productions being generally useful to society.

These two essays upon coal and the metallic mines compose the first volume.

In the second volume I proceed to take a view of the prevailing strata of Great Britain, and of many interesting phænomena of the superficies of our globe.

The philosophy or natural history of the superficies of the globe is an interesting subject to all mankind in a social state.

Many of the necessaries, and most of the conveniencies of life are found either upon or a little within the surface of the globe, being the productions of the mineral kingdom; and we are obliged to many of the strata for the plenty and excellence of our food.

Lime is of great use to meliorate the soil, and to stimulate or excite vegetation; and the gradual weathering and decomposition of the superficies of many other strata, restore and increase the soil, which may be in part exhausted or carried away by rains and currents; and if we look upon our houses, and within them, we may soon perceive how much we are indebted to the mineral kingdom.

The most remarkable phænomena which present themselves to us upon the surface, and as far as we penetrate within the surface of the globe, are remarked and explained in this third part under several heads.

1st. I have taken a view of the prevailing rocks and strata of this island, to see which of them are stratified, and which of them are not. On this head I have examined the appearance, colours, quality, thickness, regularity, bearing, slope, and course of the several classes of strata: I have collected a great number of interesting facts and local examples; and I have been at great pains to select, and to examine particularly such strata as are most useful to society.

2d. I have treated of the stratification of the superficies of our globe by the agency of water. In this disquisition the enlightened and candid naturalist will find a considerable number of

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abstruse, but interesting phænomena above ground and below, raised from obscurity, and treated of and explained upon rational principles, in a clear, convincing, and satisfactory manner.

3d. I have examined part of the modern system of Count Buffon and others upon this subject, to see how they correspond with the real structure of the superficies of the globe, and other phænomena of nature; and what I have advanced under this head will bear the severest scrutiny by every test.

4th. I have treated of the natural history of mountains, and of their glens and excavations, which is a sublime and difficult subject. In this part the height and figure of the mountains, the profundity, direction, and extent of their excavations, the exterior and interior structure, with all the most remarkable phænomena of mountains, and other irregularities of the surface of the globe, are fully accounted for and explained to a demonstration, upon the principles of the agency of water, and of the prodigious height and force of the diluvian tides; and the clear light which is thrown upon this great subject, will convince every candid naturalist of the truth of my propositions.

5th. I have examined the nature, or quality, the size, figure, and other phænomena of the larger grains and fragments which are found in the composition of our rocks and strata; and these inquiries naturally lead us into profound and interesting disquisitions relating to the universal deluge,—to the present and the antediluvian earth.

This profound and awful subject is naturally mysterious and obscure, but it has been involved in infinitely greater obscurity and confusion by the theories and systems of all ages, as the subject never has been well understood;—out of which obscurity and confusion I have endeavoured to raise it, and to explain and illustrate the doctrine of the deluge upon rational principles, agreeable to the laws and phænomena of nature.

6th. I have made a few observations concerning several other subjects relating to the mineral kingdom, among which there will be found an interesting treatise of volcanoes.

I beg leave in this place to observe, that in all this work I aim at being useful to society, especially within the limits of my own country,—my native island; but in the tract upon volcanoes my genius and imagination soars above the height of the British mountains, and takes a view of all mankind upon the whole face of the globe, and especially where they now are, or may hereafter be plagued with the dreadful calamity of volcanoes; and I hope to be the instrument of saving many lives from sudden destruction,— to mitigate the miseries and abridge common losses in volcanic countries; and if my rules and instructions for that purpose are thoroughly considered and followed,

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I am persuaded that what I have written will produce happy effects. The tract upon volcanoes is founded upon experimental science and real knowledge of natural history; and, therefore, I hope, that in time, very happy consequences will result from my essay upon this subject; in composing which, the whole powers of my soul were animated and exerted in fervent desires of doing good.

The dissertations concerning the balance of the waters of the ocean, and the accumulated mountains of ice and frozen snow, which mutually and reciprocally depend upon and illustrate one another; concerning the peopling of America by land from the north-east of Asia, and its being stocked with land animals from Armenia, in an early age, before the mountains of frozen snow were greatly accumulated;—concerning the pestilential effects of humid vapours arising from the slime of new-formed lands, from marshes and extensive woods in warm countries, and how to mitigate these dismal calamities, and to banish these undermining enemies of the human race;—concerning the deepening and improving the beds and bars of the navigable and other rivers of the world, and the draining and improving of marshes, new formed, and wood-lands, with the great and glorious consequences of such works, for the health, longevity, general happiness, and prosperity of all nations; are humbly submitted to the examination and censure of such candid and benevolent philosophers as make advances in useful improvements, and the prosperity and happiness of mankind the ultimate end of the exertion of their talents.

In these dissertations they will find many valuable hints, which they can improve, and a great deal of matter of vast importance and consequence to the health and welfare of the world, very ill put together, and in an uncouth dress, but which they may arrange, improve, and clothe in better language.

Since writing the above, and all I proposed to advance at present in the following essays, I have perused a New Theory of the Earth, by James Hutton, M.D. F.R.S. Edinburgh, concerning which I beg leave to make a few remarks in this place.

Dr. Hutton is a naturalist of eminent abilities, whose knowledge in several branches of mineralogy does honour to his country, as some of his observations in the treatise under review clearly evince. The propositions he states, with the conclusions he draws from them, to confirm his hypothesis in the theory of the earth, shall be the subject of the following observations.

The Doctor's general system in his theory of the earth may be comprised in these four propositions.

1st. That all our rocks and strata have been formed by sub-

sidence under the waters of a former ocean, from the decay of and waste of a former earth, carried down to the sea by land-floods.

2d. That these submarine rocks and strata were heated to the degree of fusion by subterraneous fire, while immersed under the waters of the ocean, by which heat and fusion the lax and porous sediment was consolidated, perfectly cemented, and all the pores and cavities filled up by the melted matter, while the whole mass was in a state of fusion.

3d. That the rocks and strata, so formed and consolidated under the waters of the ocean, were afterwards inflated and forced up from under water by the expansive power of the subterraneous fire, to the height of our habitable earth, and of the loftiest mountains upon the surface of the globe.

4th. That these operations of nature, viz. the decay and waste of the old land, the forming and consolidation of new land under the waters of the ocean, and the change of the strata now forming under water to future dry land, is a progressive work of nature, which always did, and always will go on in a perpetual succession, forming world after world.

I. The first of these propositions has been fully answered and refuted before it was written, at least before it was published, in my examination of the system of Count Buffon in his Theory of the Earth, which will be found in the second volume of my Essays upon the Mineral Kingdoms, concerning which, I will venture to say, and the candid intelligent naturalist will say with me, that I have not left the Doctor so much as a particle of earthy matter to form one of his future worlds, if a single particle would save the whole succession.

I have now effectually cut off all his supplies, and appropriated them to a better use; and I hope it will be acknowledged, that I have made a good use of them. There is little or no difference between Count Buffon and Dr. Hutton in this part of their several theories; and therefore, what I have advanced concerning Buffon's, is equally applicable to the Doctor's.

I have, in my examination of Count Buffon's Theory, frankly acknowledged the truth of almost all that the Count and the Doctor advance about the weathering, decomposition, and waste of the superficies of many of our rocks and strata, and of our mountains and cavernous shores.

The spoils of the mountains are carried down by land-floods to the valleys and to the borders of the ocean. So far we go together;—but here we must part, as I positively deny that any strata are formed under the waters of the ocean. I have, in that part of my essays, made it evident to a demonstration, that the sea purges itsel. by the tides of all the earthy matter carried

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down by the floods, which earthy matter is thrown back upon the shores, in the bays and creeks, and at the mouths of great rivers, where, by degrees, it enlarges the bounds of the dry land in exact proportion to the quantity carried down by the floods.

In that part of my essays, I have clearly demonstrated, that the earthy matter washed off the face of our mountains and rocks has no manner of tendency to the real waste and destruction of the present earth; so far from it, that on the contrary, the habitable parts of the earth are gradually, but really and effectually renovated, enlarged, and improved thereby. I have proved, that many lakes, marshes, and frightful gulphs among the mountains and in the plains, have been filled up in the course of the rivers of the world, which are now rich, beautiful and habitable countries; that many millions of acres of new land have been made in the valleys and plains, at the mouths of the rivers in the bays, creeks, and shores of the ocean; and that very many and extensive portions of this new land are now the fat valleys by the rivers, which are the scenes of population, wealth, and social happiness.

It is upon this description of land that the highest number of the great commercial cities of the world are seated; such as, for instance, London, Amsterdam, Alexandria, and many of the cities of China, &c. which have long been the seats of learning and the arts, of commerce, wealth, and glory.

Whoever will take the trouble to peruse my essays, will be convinced and satisfied that the Deltas, Belgias, and Carses, and other descriptions of new land, formed and forming in all parts of the world, fully and perfectly correspond with the quantity of matter washed off the mountains and rocks; and they will there see it clearly proved that all this is a real, a great, a substantial, and a durable improvement of the present earth.

Man cannot live upon the summits, nor high up the sides of lofty mountains; but the frosts and thaws, and other changes of the air and weather, decompose part of the superficies of the mountains, which is carried down by the floods to the valleys and to the margin of the sea, where new land is gradually increased, which enlarges the bounds of the earth in convenient situations for increased population, and for all the improvements which are necessary for increasing human and social felicity;—and are not the spoils of the mountains much better disposed of in this way, than if spread out at random through the bounds of the ocean, to form imaginary worlds in the craniums of our modern philosophers?

But this use which the wise and benevolent providence of God makes of the sediment of rivers in the ordinary course of things, is not a well fancied hypothesis, proposed for the amusement or

confusion of the inquisitive mind of man; but it is a real and visible fact, which may be viewed, examined, and thoroughly investigated by the man of leisure and abilities; and I am persuaded, that if Dr. Hutton will read my papers upon this subject, he will be convinced of the errors of his hypothesis.

Now, it being clearly demonstrated, that no strata are formed in the bed, or under the waters of the ocean, all our author's investigations and reasoning upon that subject of course fall to the ground; and I have in my essays made it evident to a demonstration, that if, for argument's sake, we allow the particles of matter carried down by the rivers to be spread out over the bounds of the ocean, and to subside in it, we should, in that event, have no coal, no limestone, freestone, nor any other useful fossil body.

We should have no such thing as strata, nor bed, nor division of any kind whatsoever, but all would be one uniform solid mass of sediment, compounded of all things. It is in vain to say it would be otherwise. The known and acknowledged laws of Nature forbid it; and all the experience we have of sediment proves the fact, that all would be a blended indistinguishable mass, as I have fully shown in my essays, to which I refer for clearing up the point under consideration. If we can suppose any order or distinction in sediment, it must agree with the laws of gravitation; of course the heaviest particles would subside, and take possession of the lowest place, from which they would not be dislodged by the lightest.

But we need not descend to particulars. Stratification must be performed [illegible] shallow spread and flow of water: but we cannot allow of [illegible]ification, nor of any distinction of strata of different qualities under the bed or waters of the ocean, without a miracle for each; and we need not have recourse to miracles, when the phænomena of Nature can be as well and better explained upon rational and mechanical principles, agreeable to the known laws and visible operations of Nature. But I will not insist upon this topic here. I have already confuted this part of the Buffonian theory, and the Huttonian differs but little from it. [To be continued.]

XXIX. *Geological Queries regarding the Strata of the Vicinity of Bridlington; and some Acknowledgements to* Nathaniel John Winch, *Esq.*, *&c.* *By* A Correspondent.

To Mr. Tilloch.

Sir, — Concerning *the causes* of the ebbing and flowing of the Spring of Water, which rises in Mr. Rennie's Bore-hole in Bridlington

Bridlington Harbour, on the coast of Yorkshire, three occasional Visitors of that place, Dr. John Storer, Mr. James Watt*, and Mr. Gavin Inglis, have offered their several *conjectures*. Mr. Milne, a resident, has done the same, and Mr. Hume has analysed its waters, without *the facts of the stratification*, of that part of the country, having sufficiently transpired, to enable myself and others of your Readers, who have never had the opportunity of examining that part, to form any safe opinion. My object therefore is now, to request the favour of *Mr. Winch*, whom I have understood to intend an examination this Summer of the northern part of the Yorkshire Coast, and I hope of this part also, and any other *practical* Investigators of the strata, that they would answer the following queries, through the medium of your pages, viz.

1st, Is the "very solid *Clay*" through which the borer passed 28 feet (vol. xlv. p. 433) in reality a bed of *alluvial Clay*?; as the bed of "cretaceous flinty Gravel," 15 feet thick, through which the borer is said to have passed, next after the Clay, may be supposed to indicate; owing to the fact, indisputable among *practical* Men, that real *Gravel*, is not found under any *regular Strata*: or,

2d, Is the Clay above mentioned, *an undisturbed stratum?*; and the flints which are mentioned, as occurring in Gravel, in reality, the fragments of nodules of Flint broken by the boring chisel, which were dispersed in the Marl or soft Chalk which was bored through, 15 feet, before a larger nodule, or a continuous bed of flint, stopped the further progress of the boring, into the Chalk Rock beneath?: or, instead of their being real Flints, which were bored up, were they not *chert nodules*, broken perhaps by the auger? and "the solid rock" which stopped the boring, concretion of a bed of the Sand, into the stone, usually called Gray Wethers?: or,

3d, Instead of the Clay which was bored through, being part of the Plastic, Potters', or Brick Clay, regularly covering the upper Chalk (sometimes without, but more commonly with, a Sand intervening) as I have supposed in the last query: may it not form a stratum, between the upper and lower Chalk?: if it be correct, that *the same stratum of Clay*, stretches up the Wolds, so as to confine down the water in the Chalk around the *Gipsies* Springs†. Because, if it be

* In the Repertory of Arts, vol. xxx. p. 342.

† Which Spring I observe, Mr. Arrowsmith's Map places, 2-3ds of a Mile NW of the Wold Cottage (where the largest British Meteoric Stone fell in 1795), and 1⅛ Mile ESE of Foxholes village, on the Hull and Scarborough Road.

correct,

correct, as Mr. Smith's Map of the Strata shows, and I have always understood from other sources, the naked *Chalk*, (and not its cover of *London Clay*) extends from Foxholes, S to Great Driffield and beyond, SE to Thornholm, and ESE almost or quite to Bridlington? &c.: the Clay around this *Gipsies* Spring, cannot be the plastic Clay above the upper Chalk; unless in a local *Trough* (which Mr. Smith has not shown) extending up the bottom of the vale from Bridlington to Foxholes?: which seems improbable, I think.

4th, What is *the Rock* spoken of by Mr. Milne (p. 434), as forming the base of the Smithwick Sand Reef, 4 miles out at Sea, SE from Bridlington, and presenting *a Cliff* under water, towards the east? Is it the upper Chalk?:—or, the Gray Wethers?:—or any of the Limestones, &c. of the Isle of Wight and Paris Series?. Concerning some of which last, so much has been *fabled* of late years, regarding their *fresh-water* origin, in distinction from the Strata in general, which have, without sufficient proof been assumed, to have had a *salt-water* origin?

5th, If "*The Gipsies*," spoken of in your last Number, p. 82, be the Spring 10½ m. WNW from Bridlington, which has been mentioned in the 3d Query, situated almost on the summit of the Wold Hills?: is it really true, that this Spring *ebbs and flows*, periodically? or is it credible, that this is anyway connected with the Tide in Bridlington Bay?—If there are other *Gipsie* Springs, much nearer to the Sea, and near to i[illegible]d, to which allusion is made?; instead of that one near [illegible]holes; where are they situated, by bearings and distances, and the streams by which they descend to the Sea?

6th, In the case last supposed, and indeed with regard to all the *Gipsie Springs*, which have so loosely been alluded to; is the superficial Clay, through which the water is said to "ooze" and "weep," around them; in reality an *alluvial* covering, to water-worn, broken and heterogeneous Gravel?; *or a stratum*, covering another porous and water-charged stratum beneath it?: and in the latter case, which are these strata, in the Smithian Series?—and whether alluvia or a stratum, is it clear, that the same extended and unbroken mass of Clay, covers the vicinities of the Gipsies and of Bridlington Bay Springs?

When the above queries are satisfactorily answered; the truth or otherwise, of the several ingenious *hypotheses* which have been advanced, with the view of explaining the alleged wonders of this

this Spring*, can better be discussed: and until this is done, as well as *the facts* of the Spring, stated on longer experience, I shall hope to see your pages, sir, more usefully occupied, than in prolonging so barren a discussion: at this day, *localized facts*, not closet speculations, on Geological subjects, are wanted, by great numbers of your Readers, as well as by

Your humble servant,

September 1, 1817. A CONSTANT READER.

P. S.—I do not feel less obliged to your able and valuable Correspondent *Mr. Winch*, for the important facts of his last Letter, in p. 122, than if the same had more directly been stated, *as corrections* of the opinions he formerly gave, when answering my Queries, (in p. 465 of your xlvith volume, p. 401 of vol. xlvii.) as also in the Geo. Trans. iv. pp. 73, 74, 75, and 76, corresponding then, nearly, with those of Dr. Thomson, as to the supposed *unconformableness*, of the masses of Basalt, scattered over the northern parts of Northumberland. Whether "the Basalt alternate with the rocks of which the whole district is composed," or not? is an important question of fact, to which my 2nd question, in p. 12 of your xlviith vol. directly went: and for the answer now obtained, I beg most sincerely to thank Mr. Winch:—the idle questions, as to whether *newest* flœtz Trap, or any others of the Geognostic *fancies*, will apply to the Strata of Northumberland, I will readily leave to Dr. Thomson and others to decide.

With regard to the last paragraph of Mr. W's Letter, I beg leave to remark, that what he truly states, as to other substances, when seen in contact with *Basalt* (both of Dykes and Strata) *sometimes* appearing *different in quality*, from the general masses of those adjacent substances: is true also, in numerous instances, which I have seen, *with regard to the contacts of several other substances* filling Dykes, or forming immediate alternations of strata, without the intervention of the Wayboards or *partings*, which more commonly are interposed: and, that instead of considering, in such situations, the Slate Clay as *turned into* flinty slate, &c. the Coal as *being charred*, the Sandstone, as *changed*, to a brick red, and the Limestone as *rendered* highly crystalline, &c. *by changes* wrought on these masses, subsequent to their original formation, *by heat*, communicated to them from the Basalt when in a melted or Lava state?—on the contrary, I have seen, such abundant reasons for considering all these *alleged changes*, and many others, as blendings, or infiltrations of the component substances of the adjacent masses, coeval with the

* *Brighton*, in Sussex, had in like manner its wonderful Wells, until 1802, when their mysteries were cleared up; see Nicholson's Journal, 8vo, iii. 65.

formation

formation of one of them:—or, as the consequence of a subsequent chemical decomposition of one of the surfaces in contact: —that I cannot doubt, if it could so occur, that Mr. W. or any others of similar ways of thinking on this point, could conduct me to the very strongest case in Great Britain, of their alleged *charring or changing* of adjacent substances, by *the heat of Basalt:* I could point out facts *on the spot*, which would completely overturn such a supposition;—with *hand Specimens*, theoretically selected, or with *descriptions* by others, so tinctured, the result might possibly be otherwise. This test, our *theoretic inferences* must bear, in every instance, if they are worth anything, or worthy of being communicated to others, or remembered.

I have already and fully explained myself, in p. 253 of your last volume and elsewhere, as to the locally *variable thicknesses*, of continuous strata of Basalt, forming what may be considered as somewhat irregular *lenticular masses*, either plano- or double convex; surely Mr. W. will on reconsideration agree with me in thinking, that "wedge-shaped masses," but inaptly designates them. I hope that none of your succeeding Numbers, for some time, will appear without communications from Mr. Winch, Mr. Forster, Mr. Fryer, or some other industrious Observers of *the Geological facts*, of the northern English Counties, disposed to freely communicate what they know.

2d P.S.—I heartily wish that Mr. Winch, or his Friend to whom he alludes in your last, would send up to *Mr. Sowerby* (No. 2, Mead-place, Lambeth) ample Specimens of all the kinds of *Shells*, found in the Limestone of Wratcliff, or in any other Quarries, with their precise localities marked; in order that in future Numbers of his "*Mineral Conchology*," they may be drawn, described, named and compared, with other distant localities of *the same species* of Shells. A. C. R.

XXX. *On the Rotary and Orbicular Motions of the Earth.* By *Mr.* H. Russel.

To Mr. Tilloch.

Sir,—I will esteem it a favour if you will give a place to the following letter, in your publication,—and am, &c.

Norwich, July 24, 1817. Henry Russel.

"*To Sir Richard Phillips.*

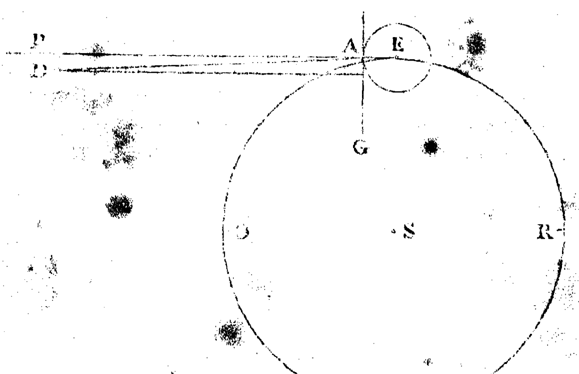
"Sir,—To account for the attraction of gravitation, has long been an object of my most serious inquiry, and I am sorry I cannot find in your paper (of last June) that gratification which by the

the title I was led to expect. I cannot conceive what could induce you to suppose, that the orbicular and rotary motions of the earth, are the cause of that great principal attraction, of which you justly observe, the Newtonians and all the modern schools of philosophy have acknowledged themselves ignorant.

"I think it is very easy to show, that these motions, which are themselves only effects, cannot be the cause of what in every point of view appears to be a first principle. If you were to attempt an illustration of your theory by actual experiment, I am persuaded you would discover its fallacy.

A circular plane surface, ten or twelve inches in diameter, lying in the plane of our horizon, with grooves cut in its upper surface on lines drawn from the centre to the circumference, might have a rotary motion given to it, and if globules of mercury were put into the grooves, the centrifugal force would by them be exhibited, and you would find that no orbicular or any other motion, that you could communicate, would be able to bring all the globules of mercury at the same time to or towards the centre, which, if your doctrine was true, would undoubtedly be effected by giving it a circular motion, similar to the motion of our earth in its orbit.

I should very much like that you would try this, or some other experiment, by way of illustration, before you apply your "principles to the phænomena of a system of bodies moving within the gaseous medium of universal space."



Let the circle O R represent the orbit of our earth; S the sun in the centre; E the earth; P E, a line drawn from the centre of the earth through the point of projection; T G a tangent of the earth; A D a diagonal of the rectangle D P A, the longer sides of which are to the shorter, as the orbicular motion is

is to the rotary, or as eighty to one. Let us suppose the axis of the earth perpendicular to the plane of the earth's orbit, that the earth is turning from A to G, and that it moves in the orbit in the direction EO. A stone projected from the piont A, will continue to rise till its *vis inertiæ* is overcome by the attraction of gravitation, by which it will be drawn to the point from whence it was projected. The orbicular and rotary motions of the earth, have no power, whatever, to cause a body thus projected, to return again to the earth; but on the contrary, were it possible that the earth could perform its revolutions, rotary and orbicular, without the existing principle attraction the stone spoken of, without the addition of any muscular or explosive force, would not remain on the earth, but would fly off in the direction AD, in obedience to the indisputable laws of motion. An attentive examination of the annexed diagram, will familiarly show that it is impossible for a projectile thus neglected by its guardian attraction, ever again to return.

I am willing to admit, that the orbicular and rotary motions of the earth combined, on account of the inclination of the earth's axis, produce some peculiar effects not yet justly noticed; but I am more inclined to suppose that they are the cause of the precession of the equinoxes, or of the nutation of the earth's axis, than of that great and still unfathomable principle which cannot but excite the wonder and admiration of unassuming philosophers.

But admitting (which I have not the least inclination to do) that your theory holds good at the equator, how will you account for the attraction of gravitation at or near the poles? How will you account for the horizontal attraction of the sun and moon? will you be able to account for our tides, neap and spring? If you can give satisfactory answers to these questions, you will no doubt very much stagger the present ideas of,

Sir, yours, &c.

Norwich, July 24, 1817. HENRY RUSSEL.

XXXI. *On Mr.* TATUM'S *Experiments on Vegetation.* By A CORRESPONDENT.

To Mr. Tilloch.

SIR, — IN the advanced state of chemical science, the accumulation of experiments proceeds with so much rapidity, that it is possible a man of the most extensive reading may claim as a discovery an observation which had been made by another. But when a correspondent pretends to enlighten one of the most controverted subjects of experimental science by views and experiments

periments which have been detailed in half a dozen professed treatises, and otherwise promulgated in every possible way, he surely betrays a most unpardonable ignorance. The correspondent to whom I allude is Mr. Tatum, who has favoured you with a paper in a late Number, wherein he alludes to the old story of the purification of the atmosphere by vegetable respiration, of which he says, few or none doubt the correctness. Mr. Tatum however could not rest satisfied with the general adoption of this opinion, and in the true spirit of philosophic research he determined to try the matter himself. Accordingly his experiments teach him that seeds, when confined under a jar, evolve during germination only carbonic acid; and he moreover *discovers* that plants in common with animals consume the oxygen of the air, which is accounted for in the formation of carbonic acid. These facts no doubt would be very interesting discoveries, had they not been discoveries of twenty years standing. I have said that Mr. Tatum's observations have been anticipated by half a dozen authors, and I think I shall be able to make good the assertion. The opinion that plants purify the air originated, as is well known, with Dr. Priestley; but even he seems afterwards to have been aware of the inaccuracy of his conclusions;—"for," says he, in vol. iii. p.273, "in general, the experiments of this year were unfavourable to my *former* hypothesis,—for whether I made the experiments with air injured by respiration, the burning of candles or any other phlogistic process, it did not grow better, but worse; and the longer the plants continued in air the more phlogisticated it was. I also tried a great variety of plants with no better success." The first author that *experimentally contradicted* this opinion was Scheele; and to avoid prolonging this letter, I shall content myself with referring to his work on Fire and Air, p.160. After Scheele came Ingenhousz and Sennebier, one of whom wrote three volumes of experiments on this subject; the other, five. That Mr. Tatum may lose no time in looking over the ill-digested works of these authors, I refer him to Ingenhousz's book, vol. i. p. 255; and again, vol. ii. p. 758, and to vol. iii. p. 114, of Sennebier's publication, *Physiolog. Veget.* at which references he will find an explicit declaration of what I have said. At present we have still living M. Saussure junior, who has written a most interesting, ingenious, and luminous work on the Chemical Functions of Vegetables, and his experiments entirely corroborate what had been done by Scheele, Ingenhousz and Sennebier. Vide *Annales de Chimie*, tom. xxiv. p. 139, and his work entitled *Expérience sur la Végétation*. Before the appearance of Saussure's work the attention of the public was called to this question by the first volume of Mr. Ellis's treatise on the Respiration of Plants and Animals,

Animals, in which he faithfully notices all that had been done by his predecessors, and establishes the point by abundant research, that the whole of animated nature, whether vegetable or animal, abstracts the oxygen of the air, which is entirely bestowed in the production of carbonic acid. In Mr. Ellis's second volume (a most elaborate and interesting performance, and the latest work on the subject) Mr. Tatum will find the question resumed; and that while Mr. Ellis maintains that carbonic acid is the result of the natural respiration of plants, he proves that there is a second function, by which, during bright sunshine, the carbonic acid so formed is reconverted into oxygen. This process, he contends, is entirely a chemical one, depending on the chemical agency of light, and by no means to be considered as a necessary or natural operation. Thus far and much other interesting matter, with regard to the difference of colour in different plants and at different times of the year, Mr. Ellis has ably established. The question still remaining is, not whether plants have the power of counteracting the vitiation produced by the breathing of animals; but whether they are able during sunshine to reconvert into oxygen the carbonic acid they form during darkness and common daylight. The solution of this question I have attempted, and I hope one day to give a satisfactory answer to it. The sixth author who has touched on this question is Sir H. Davy, in his Agricultural Chemistry, who details two experiments which he made in order to convince himself that Mr Ellis had not been deceived by his extensive researches.

Independently of these works Mr. Tatum will find an analysis and critique of Mr. Ellis's opinion in the Quarterly Review; and the subject is also fully discussed in Murray's and Thomson's Systems of Chemistry. I conclude by saying, that all Mr. T's experiments have been executed before, and some of them a dozen times over.

I am, sir, Yours respectfully,

W. H. G.

XXXII. *Remarks on Sir* R. Phillips's *Defence of his Hypothesis. By Mr.* Thomas Tredgold.

To Mr. Tilloch.

Sir,—As Sir R. Phillips has favoured *some* of my remarks on his hypothesis with a reply, I will endeavour once more to convince him of the fallacy of the opinions he has put forth.

Sir Richard appeals to experience, without once bringing forward an experiment to prove the correctness of his views; and to the laws of Newton and of Nature, without once showing that they

they agree with his hypothesis: hence, it is difficult to refute such undefined opinions. Undoubtedly Sir R. thinks that he has corrected the mistake, and therefore rendered the demonstration in the Phil. Mag. for July, p. 436, correct; in this, however, he is mistaken. The demonstration is intended to prove that bodies are deflected towards the earth by a power which decreases inversely as the square of the distance. Now it is evident, that a projectile or mass of matter can be acted upon by that part of the spheric surface only which it occupies; also, that it cannot occupy similar portions of spheric surfaces; —but it is similar portions only that are to one another as the squares of their radii: therefore, the conclusion is equally as incorrect as it was before. And, as a proof that circular motion has not any effect to impel a body towards the centre of motion; place an open vessel of water upon the internal part of the rim of a wheel, and turn the wheel with considerable velocity; when the water will acquire such a degree of centrifugal force as will retain it in the vessel, in all positions of the wheel. Again, if a ball or other body were made fast upon the external part of the rim of the wheel, and it were put in motion with a considerable velocity; then could the power that confines the ball to the wheel be suddenly removed, the ball would fly off in a tangent to the rim of the wheel.

As either of these experiments might be tried without much difficulty, I would recommend them to Sir R's notice: though it be now too late to save him from exposing his ignorance of the laws of motion, (see his answers to the second and third objections,) it may prevent a repetition of a like exposure.

Sir Richard has certainly adopted a very singular mode of defending himself; for he assumes the most questionable part of his hypothesis, to be an established truth, (viz. the deflective power of the rotary motion,) and then proceeds to reply to the minor objections, by telling us, over and over again, that the deflective power of the rotary motion is equivalent to gravitation.—Of course, if that were the case, it would produce the same effects. But Sir R. has not anywhere shown that it is equivalent—nay, not even that it has the least tendency to deflect a projectile towards the earth.

If Sir R. would take the trouble to define the sense in which he uses the word motion, it would then be a little more clear how far it is better known than gravitation. According to the common definition of the term, motion is only an effect; of which it is the object of the philosopher to inquire the cause.

Newton and others have shown gravitation to be one of the causes of the rotary and orbicular motions of the planets, of the flux and reflux of the ocean, the descent of projectiles, and

various other phænomena. Sir R. Phillips attempts to show that one of these effects is the *cause* of the other; and calls this advancing human knowledge a step further—I suppose he means backwards; therefore I will leave him to pursue the course he has chosen.

I am, sir, yours, &c.

THOMAS TREDGOLD.

XXXIII. *On Sir* RICHARD PHILLIPS's *supposed Discovery of the Cause of the Phænomena of Terrestrial Gravitation.*

To Mr. Tilloch.

SIR, — To appreciate the success with which Sir Richard Phillips has defended his *discovery of the cause of the phænomena of terrestrial gravitation*, it might, perhaps, be sufficient to remark, that he has left untouched Mr. Tredgold's fundamental objection; viz. that as neither the resistance of the atmosphere, nor the motions of the earth on its axis, or in its orbit, have separately any tendency to deflect towards its surface, bodies projected upwards, it follows that their conjoint action can have no such tendency. Permit me, however, by way of commentary, to add, that it has long since been demonstrated, that whether a body be projected by a single impulse, or by many simultaneous impulses in different directions, the progressive motion communicated must be rectilinear. The combination, therefore, of the two-fold motion of the earth with any other impulse, can, in projecting a body, impress on it no other than rectilinear progressive motion; nor can these forces, after the instant of their joint impulse, in any way modify the direction then impressed. There remains, therefore, of Sir Richard Phillips's forces only the agency of the atmosphere to deflect the projectile from a right line into such a curve as must return to the earth. Now the effect of atmospheric resistance would be that of simple retardation, did not the rapidity of the rotary motion of the parts of the atmosphere augment with their altitude. A consequence of this circumstance is, indeed, a continual deflection of the projectile from its initial direction: but whatever deflective force may be assigned to this cause, it can never make a body describe a curve returning to, or even approaching the earth's surface; for the very obvious reason that the direction of its action must always be parallel to tangents of that surface.

This being so, the theory of Sir Richard Phillips does not precisely correspond with his description of it as "a theory which substitutes *the known motions of Nature* as operative causes of certain physical phænomena in place of *an assumed principle*

principle called gravitation, by which, false analogies have been introduced into philosophy." Let us, however, in a single instance observe how these "*known motions of Nature*" supply the place of the "*assumed principle called gravitation*."

The weight of bodies, and their velocity in falling, uniform experience shows to be *least at the equator*, and to *increase* with the latitude. Now as the rotary motion of the earth's surface and atmosphere diminishes from the equator to the poles, where it ceases in both; the weight of bodies and their velocity in falling, ought, according to Sir Richard's doctrine, to be *greatest at the equator*, and to *diminish* as the latitude increases. Nay further, since there exists neither rotary motion on the earth's surface at the poles, nor in the atmosphere in its prolonged axis, a direct consequence of his *doctrine* is, that *bodies at the poles are devoid of all weight*, and when projected perpendicularly thence, *they never return to the earth!*

I am, sir,

Your very obedient servant,

Bath, Sept. 9, 1817. F. E . . . s.

XXXIV. *The Description of a Safety Furnace for preventing Explosions in Coal-Mines.* By Robert Bakewell, Esq.

To Mr. Tilloch.

Dear Sir,—The attention of coal proprietors has lately been directed to the explosions which take place in mines from the lights used by the workmen; but it is well known that similar explosions are often occasioned by the inflammation of the carburetted hydrogen gas, as it passes the fire placed near the upcast shaft to rarefy the air and promote ventilation. The fire which is generally necessary, is thus not unfrequently the cause of the most fatal accidents.

To prevent this, no remedy has been proposed that I am acquainted with, except the substitution of a charcoal fire, on the principle that the gas will not explode by a red heat burning without flame. An open charcoal fire is, however, liable to the following objections. The carbonic acid gas generated by the combustion of charcoal being specifically heavier than the air of the mine, will, as it is cooled in its passage upwards, descend again and choak the lower part of the shaft. A particle of common coal intermixed with the charcoal, or falling into the fire, might produce flame and cause an explosion. The comparative dearness of charcoal will also tend to prevent its introduction. Coke from coal is more easily procured, but it sometimes beams

 with

with a lambent flame sufficient to ignite explosive mixtures of gas.

A fire which will burn with perfect safety in mines, and at the same time occasion a brisk circulation of air, has been hitherto a desideratum. I am induced to believe, that I have discovered how this may be obtained by the introduction of a furnace, simple in construction, in which coke or even common coal may be burnt, without any danger, and the circulation of air in the mine greatly increased.

The furnace or stove admits of considerable variation in size and form, according to the situations in which it may be used; and as it can be erected at a small expense, I trust no prejudice can exist to prevent a fair trial of its utility. The accompanying drawing will explain its construction, and enable any coal proprietor to apply the furnace to his own use. AA *aa* (Plate III. fig. 1,) represent the body of the stove and chimney, which should be of cast iron in one piece without any side-door or opening whatever, as it is supplied with fuel at the mouth or chimney. *bb* represent the grate, which moves upon an hinge, and opens downwards by removing an iron peg *c*, in order to clear the grate when wanted, and *dd* a broad rim below the grate perforated by the air-holes *eee.* F is a cistern of stone or brick to be filled with water above the lower edge of the rim *dd*. The diameter of the furnace at the grate may be 30 inches, that of the chimney about 18 inches, to admit the fuel. The height of the chimney, if coke be used, need scarcely exceed ten feet, and may be inclined or not at option, according to the situation. When the furnace is lighted, which may be done by introducing lighted coke down the chimney, there will be no access of air but through the apertures *e e e*, which may be regulated at pleasure, by stops to increase or diminish the current. The use of the water-trough is to confine the admission of air to the openings *e e*, and also to prevent any accidental inflammation of coal or other substance below the grate.

Should the air of the mine be charged with inflammable gas to the explosive point, it is prevented from passing near the surface of the fire by the sides of the stove; and should even the lowest stratum of air which enters the apertures *e e* be explosive, which can rarely occur, this air will lose a part of its oxygen by passing through the red-hot coke, and by its further admixture with carbonic acid gas confined in the stove, must cease to be inflammable. The general current of inflammable air will pass with perfect safety over the mouth of the chimney, and will ascend the shaft from the heat communicated to it by the surface of the stove. If common coal be used, a chimney of greater length

length must be joined to *a a*, reaching beyond the possible extension of the flame. To prevent inflammation at the lower part of the fire, there must be only one aperture below the grate, into which an air-pipe must be closely fitted. This must extend above the top of the pit, and be of sufficient width to admit a free passage for the air downwards to the fire. By this means the remotest possibility of danger would be avoided either from a coke or a coal fire; the rarefaction of the air would be dependent on the heated surface of the stove.

I have ascertained by experiment, that a small current of pure hydrogen gas will inflame at the distance of nearly two inches above the apex of a newly-snuffed candle, but will not inflame when passed over the chimney of a lamp where paper would be scorched without ignition. We may by this means have a metre of the length of chimney necessary to prevent the inflammation of gas, according as the materials burned may evolve more or less flame.

Where the apertures *e e* are used without the air-pipe, they may be covered with double wire-gauze, which might prevent any inflammation under the grate communicating with the air of the mine. With these precautions one or more fires might be constantly burning near the upcast shaft, and by increasing the quantity of heated surface, we may accelerate the ascent of air more rapidly than by an open fire as at present used.

So long as the fire continues to burn, the air in the mine will never pass down the chimney or reach the fire from above; and were the hydrogen to inflame when the air is admitted through the apertures *e e*; if they are clothed with wire-gauze, the flame will be confined to the under part of the grate, and may be instantly extinguished by closing the apertures.

The simplicity of this safety furnace will, I trust, recommend it to the early notice of coal proprietors; and should it be found to lessen the dangers to which the workmen in mines are exposed, my object in this communication to your valuable publication will be fully answered.

I am, dear sir, yours, &c.

13, Tavistock Street, Bedford Square, Sept. 5, 1817. ROBT. BAKEWELL.

XXXV. *Remarks on Mr.* MURRAY'S *Objections to Experiments on Vegetation detailed in the* Phil. Mag. *for July last.* *By Mr.* J. TATUM.

To Mr. Tilloch.

SIR, — YOUR correspondent Mr. Murray has, in your last Number, objected not only to the manner in which I conducted the

the experiments relative to the effects of vegetation, &c. on atmospherical air, communicated to your Magazine of July; but also to those of Mr. Ellis, which he says "are as liable to objections as any other."

He disapproves of the "mercurial effluvia," without proving that such existed in the experiment, at least so as to retard the functions of the plant, for there was but about two or three inches surface of mercury exposed to the air of the plant (but as I believe Mr. Ellis did not use mercury in his experiments, he is not liable to this objection). To the "confined instead of a free atmosphere" he also objects; and then concludes by stating, that he holds unchanged "the opinion he has long maintained as to the *direct* experiments of Drs. Priestley and Ingenhousz, since corroborated, namely, that the quantity of carbonic acid evolved by plants will bear but a *pitiful* proportion to the *floods* of oxygen poured out upon the atmosphere by the exercise of the vegetable function."

Now, I would ask the objector what were the *direct* experiments of Drs. Ingenhousz and Priestley, and by *whom* and *how* since confirmed, which have so confirmed or established him in his opinion? Was it the experiment of Dr. Ingenhousz, as related at page 14 of his work? where he says: "they (the detached leaves) are to be put in a very transparent glass vessel, or jar, filled with fresh pump water, (which seems best adapted to promote this operation of the leaves, or at least not to obstruct it,) which being inverted in a tub full of the same water are to be exposed to the sunshine: thus the leaves continuing to live, continue also to perform the office they performed out of the water."

Is this experiment of the *unnatural* situation of *detached* leaves *less* objectionable than the one in which an entire plant, or spring while attached to its parent, is placed in atmospherical air? Should Mr. Murray think so, I shall still, whenever *I* wish to ascertain the effects of a plant (not an aquatic one) on the *atmosphere*, always place it in *atmospherical* air, and *not in water*; and shall always prefer using an entire plant, or that part of one attached to its parent, rather than *detached* leaves.

As for the "*floods* of oxygen" which he says are "poured out upon the atmosphere by vegetation, being so superior to the *pitiful* quantity of carbonic acid; this remains to be proved; for I do not recollect one experiment either of Drs. Ingenhousz, Priestley or others, which is adequate to it. And although he is such a strenuous advocate of Dr. Priestley, he must acknowledge that the Doctor's experiments frequently proved the contrary; for at p. 338 of his third volume, the Doctor states "that the air in which a willow plant was growing, continued to *de-*

crease

crease in purity for *twenty days*." And at p. 278, vol. iii. the Doctor says: "the experiments of this *year* 1778, to which I had been induced to pay *more particular attention and care*, were *unfavourable to my former hypothesis*." I could adduce more experiments of the Doctor's, of this nature, but think these will suffice.

By *whom* and by *what experiments* the doctrine he maintains has been "since confirmed," I know not, unless the experiments of Sir Humphry Davy are alluded to.

But as Mr. Murray objects to *my* experiments being conducted in a *confined portion of air*, I must remind him that Drs. Ingenhousz, Priestley, and Sir H. Davy's were all performed either under water or in a confined portion of air. But perhaps he can approve of that in Drs. Priestley, Ingenhousz, and Sir H. Davy, which his *confirmed* opinion will not allow him to do with respect to Mr. Tatum. If his object be the support of truth, I hope he will not suffer himself to be influenced by partiality, or *names*. I have, it is true, presumed to differ from the above highly respectable characters; but I have yet to learn, by what means any of these experimenters ascertained the results of vegetation on air without its being "confined;" and I hope Mr. Murray will have the goodness to inform me, by what *peculiar* plan he has discovered that vegetables pour out such *floods* of oxygen, and he may rest assured I will lose no time in adopting it; and he will find me far from being backward to give him all the praise.

Mr. Murray asks, "If the carbonic gas was at all equivalent to the oxygen *set free*, whence comes the carbon which builds up the plant?"

I cannot think this question can possibly apply to any thing related in my paper; for I there contended that oxygen was *not set free*, as such I cannot comprehend the object of the question. He proceeds to say: "the winter no longer contributing the air necessary to life in Europe, the salutary gas is brought to us by the trade winds from the southern regions."

Really, sir, Mr. M. has drunk deep of Dr. Priestley's principles; for the Doctor entertained unnecessary apprehensions of a deficiency of oxygen for respiration, and sought for a supply, which he said he "found in vegetation." So Mr. M. equally apprehensive that the *floods poured* forth in Europe, would not be equal to the consumption, imports it from the southern regions. But having heard much talk of the superiority of the oxygen of the atmosphere at some parts, over that of others, I was induced to ascertain whether such was the case; but as yet I have not been able to discover it, and I find I am not solitary in my results; for Sir H. Davy could not distinguish any difference between the air brought from Guinea and that of Bristol; and how

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the gentleman has ascertained by *direct experiment* that such a redundancy of salubrious air exists in the southern regions, I am at a loss to know. I hope he will have the goodness to point out the plan by which he ascertained such an important phænomenon; which will confer a favour on,

Sir, yours, &c.

Dorset Street, Salisbury Square, Sept. 9. 1817. J. TATUM.

P. S.—Any hints from your correspondent Mr. J. Acton will confer a favour, as perhaps I shall pursue the subject next spring.

XXXVI. *Answer to Geological Queries of "A Constant Reader."* By *Mr.* WESTGARTH FORTER.

To Mr. Tilloch.

SIR, — I HAVE observed in your Number for July last, some geological queries, by your correspondent, with the signature of "A Constant Reader," requesting an early answer thereto.

I therefore take the opportunity of informing him, with respect to his 2d question, (" whether or not the *great whin sill*, or stratum of basalt, shown in p. 152 of my Treatise on a Section of the Strata, &c. has not such a *continuous edge* on the surface, as clearly indicates it to form, like each of the other principal strata, a vast *extended plane* having curved parts within the earth, &c.") That wherever I have made observations, or traced the great whin-sill, it is as conformable as any other stratum, although *very variable in thickness*, having its under lieing and over lieing strata. And it may be traced upwards of fifteen miles, commencing a little below the smelting-house at Tynehead, where it is thrown up to the SW. about sixteen fathoms, by the influence of a great dyke or vein, commonly called the *Back-bone, or Great Sulphur Vein.* As we proceed, a little above the smelting-house, it disappears, about the distance of two miles, having its over lieing stratum upon it; viz. *Tyne-bottom limestone, &c.* until we arrive at the river Tees; where it again makes its appearance, the Tees running upon it almost all the way to the high waterfall at Caldron Snout, where it assumes locally, *the appearance of detached and over lieing masses of basalt;* which, as Mr. Winch observes, may very probably resemble those of the King's Park Edinburgh.* It may be also necessary to state, there is a level drove in the limestone, under the great-whin-sill, not far from Caldron Snout, and near the conflux of the river Tees, and Maize-Beck; and this Beck, or rivulet, which divides the counties of Westmoreland and Yorkshire, near Birdale, runs all the way for the distance of a mile or more,

more, W. of the conflux, upon or even, in the whin, where it again disappears, by the over-lieing of *Tyne-bottom limestone,* which limestone may be traced to the W. up the same Beck, to *High Cup Nick,* where the stratum becomes abrupt, as we descend to Dufton, and the whin, basseting underneath, and only about eight fathoms thick. It may also be observed at *Great Rundle Beck,* where the principal level commences, and is drove upon it, to the mines at Dufton-fell.

I may further add, that the same great whinstone-bed occurs on the river Wear, near the town of Stanhope, in the county of Durham; but not so thick as at Caldron Snout.

I shall endeavour to answer the other queries in my next communication.

I am, sir,

Your most obedient servant,

Ganigill, August 26, 1817. WESTGARTH FORSTER.

XXXVII. *Description of an Apparatus for consuming Fire-damp in the Mines without Danger of an Explosion:—Apparatus for re-lighting the Miners' Davy.* By Mr. J. MURRAY.

To Mr. Tilloch.

SIR, — THE sketches which accompany this, represent an apparatus by which the fire-damp may be consumed on the spot, in the mine, without fear of explosion; and an appendage to light the safe-lamp when extinguished. It is presumed that the descriptive account will be found sufficiently explanatory, and intelligible. If it should be objected to (fig. 1), that its size is too great,—it may be observed, that in the Air collieries safe-lamps on the principle of the wire-gauze have been used by Mr. Taylor *three times* the size of those constructed by Sir H. Davy.

I did not find the plan I originally proposed to relight the lamp by any means unequivocal, when tried in the mine. This circumstance led to the present invention.

I am, sir,

Your most humble servant,

Douglas, Isle of Man, Sep. 3, 1817. J. MURRAY.

P. S.—The great increase of intensity which I discovered by exposing the Galvanic plates for a few minutes to the action of the atmosphere, prior to reimmersion into the cells, I have since repeated very often with the same uniform results. I shall be glad to see these in your next Number.

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Description of the Figures.

Fig. 2 (Plate III.) shows a cast-iron urn resting in a vessel of lime-water, or cream of lime, to absorb the carbonic acid gas formed. It is topped with two folds of wire-gauze at A. At BBB are three or more sockets, the orifices of which are covered with wire-gauze. In these sockets are fixed tubes of tin C, which move up and down to any height like the sliding pipes of a perspective or opera glass; they terminate in a funnel-shaped orifice, or they may be jointed, to incline at any angle to receive the explosive medium for combustion. The "Davy" will be the index of the requisite height; the tubes should be raised within half an inch of the base of the fire-damp, floating on the roof, so that a due admixture of inflammable air and its supporter may enter the funnel of the tube, together.

Fig. 3 exhibits a view of the internal insulated wire-gauze, being that which imprisons the wick of flame; it does not touch the surrounding cast-iron case or urn, nor even the upper wire-gauze, on its top. This cage may be made two or three folds. The lamp is fed by an oil cistern exterior to the urn, and a fold or two of wire-gauze in the communicating pipe will prevent any retrogression of flame where the cistern is to be supplied with oil. The wick may be of *asbestos*, which will never need renewal; and the lamp, first lighted, is screwed tight into the socket.

Fig. 4 represents the oil cistern of Davy's safe-lamp with two separate wicks. A exhibits one of these wicks surrounded by a platinum cage. B the *reserve* wick, with an appendage which serves at once to elevate the cap and depress the spiral platinum wire to ignite the wick; *a* the cap attached to the axis *f* by the wire *b*. *c* a spring, that when at freedom reacts on the wire attached to the cap, which then falls and protects the wick, when not required; *d* is a spiral platinum wire attached by *e* to the axis *f*, which moves by a button exterior to the wire-gauze. The reserve wick is tipped with *sulphur*. When the wick of the lamp A is extinguished by reason of excess of fire-damp, the singular combustion of the platinum wire begins, and continues until there exists no longer any of the hydrocarbonate. The platinum wire before extinct becomes dull red; this will indicate an approach to the free atmosphere the moment after, by a semirotatory movement of the button, the cap is raised, and the top of the platinum brought in contact with the wick tipped with sulphur, which ignites it.

XXXVIII. *On*

XXXVIII. *On the new Theory of the System of the Universe.* *By* Sir RICHARD PHILLIPS.

THE theory which ascribes the subordinate motions on the earth to its superior motions as a planet, is opposed by many persons, who, assuming that the motions of the planets in a system are nevertheless governed by gravitation, ascribe incongruity to a new doctrine which excludes that principle from the internal or local phænomena of a planet.

The author of that theory is, however, for good and substantial reasons, of a totally different opinion. He believes in the perfect harmony of nature—in the exact analogy of causes and effects—and, wherever he sees motion, he ascribes it to other motion ascending in a series *ad infinitum*, or to AN UNKNOWN CAUSE. He therefore gives no credit whatever to the existence of any universal principle of causation, such as that called by the name of gravitation, but refers all phænomena to motion, primarily and proximately.

He was not anxious at present to press this extension of his theory on the world, because it is less easy to demonstrate that distant planets move one another by impulse, than it is to show that loose bodies in a ship, or on the earth, are governed in their subordinate phænomena by the paramount motions of the ship or earth. Every one capable of understanding its terms must feel as an axiom, *that the orbicular and rotary motions of the earth necessarily give weight to bodies, and laws to their fall, because the moving earth and the bodies are in contact, and partaking of those common motions*; but certain postulata must be granted before it can be proved to beings whose experience is confined to the subordinate phænomena of the earth, that disunited planets and masses can operate on each other mechanically, and communicate motion to one another.

The postulata required to be admitted are as under:—

1. *That all space is filled with some gaseous medium.*

In the age of Kepler and Newton, the discoveries of Priestley had not proved the existence of various gases. An incompressible fluid, so light as hydrogen, was not then known to exist. The similar phænomena of the planets; the combustion of meteors at great heights; the transmission of solar and planetary light, and the reflection of the solar light after it has been refracted through the atmosphere of a comet, prove, however, that some rare medium actually fills space; even if its existence were not sufficiently proved by the mechanical phænomena of the planets.

2. *The medium of space is acted upon in straight lines by moving bodies placed within it.*

It is difficult for men who are accustomed to see the connexion

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of rods or levers of fixed continuous matter between bodies acting on one another, to conceive that any gas, like hydrogen, can act by like agency. But this power of gas will be evident on slight consideration; for, if a tube, or series of tubes, of ten feet or a million of feet in length, were filled with hydrogen gas, and a plug were driven into one end, so as to require any known power less than the strength of the tubes to force it out; then, if a piston were forced with that degree of power into the other end, it is notorious that the rarest gas would expel the plug as effectually as though it were propelled by a continuous rod of iron. If space, therefore, be full of any light gas, or fluid *sui generis*, it is evident that such gas, in such a plenum, must act in continuity in filled space, as well as in a filled tube. We know that the gas in which we live acts thus at definite distances, in proportion to the closeness of the place of experiment; and we must not forget, that in the only situation in which a good experiment could be made, the effect of this continuous power in mere gas was very remarkable: viz. when Blanchard and Jeffreys crossed the Straits of Dover, they threw from their car, when at the elevation of two miles, an empty bottle, the fall of which on the water produced *a sharp concussion* in the car, thereby affording proof of the continued impulse of gas, even when the impulse is made in free space. The ascent of sound, and its propagation through distances of three or four hundred miles near the earth, is a further proof of such capability, though the vibrations of sound are not exactly of the same nature as the propulsion of impulse.

COROLLARY.—*This important consequence follows, that, as impulses in a gaseous medium must act in cones diverging from the moving power, so the force of the impulse must necessarily diminish as the squares of the distance; the impulse from a focus through gas being of the nature of the impulse of light, heat, and all emanations.*

These are the postulata on which I propose to raise a new theory of the universe, without the aid of gravitation.—And on these bases it cannot be difficult so to combine the laws of motion as to account for all the ordinary phænomena of the universe.

In such considerations, the governing principle is an exact fitness and harmony between causes and effect; and these impose the necessity of a balance of powers. *A balance of powers requires, however, equal momenta; and equal momenta grow out of equal quantities of motion, on two sides of a fulcrum, centre, or axis.*

In Universal Nature there is no up nor down; there is no natural disposition of bodies to fall together, or to recede from one another;

another; and no phænomenon is produced but by analogous causes exactly equal to the effect. Thus motion necessarily produces motion, and the existence of motion affords proof of the existence of a cause in some superior motion. Disturbance is always counteracted by the inertia of matter, and the mutual contest between the moving agent and the moved patient, causes both to turn round the centres of their masses, or round a fulcrum, on each side of which the quantities of motion are forced to seek equality.

In the solar system, the sun is the moving power of all the planets. Whatever be the origin of its own motions, the SUN acts, in the œconomy of the planetary bodies of the *solar* system, like the HEART in the œconomy of the *animal* system. Its own motion may be created by some arrangement within itself—by a perpetual motion of divine contrivance—by the cross and reciprocal actions of the planets—or, according to an hypothesis of Herschel, it may have a superior orbit among systems of suns; and our planets and their satellites may be its secondaries and sub-secondaries! It will, however, satisfy the spirit of philosophy, if we can trace all those motions, which have hitherto baffled inquiry, to the natural action of a *primum mobile* like the sun; and we may be content there to terminate our inquiries, at least for some ages. Thus much seems certain, that the motions of the solar system may be correctly likened to that of a pentagraph or polygraph—the planets mimicking the motions of the central mass, just as the tracing points mimick those of the original in the action of that machine; or perhaps the motion of the sun may be compared to that of the hand, while whirling a string with a weight at the end—the hand moving through a circle of one or two inches, giving thereby an orbit of several yards to the weight at the end of the string. In universal space, however, and in performing absolute motion, the planets move in no relations like that of the weight to local and relative powers; and therefore have no inclination to fly off in a tangent * !

In tracing the effects from their causes, let us suppose the solar system to be stationary: let the sun, whose mass is a given number of times greater than either of the planets, be moved one foot—then will each of the planets be moved in the same direction, according to a ratio governed by the positions and bulks of the whole, a certain number of feet, as 100,000, or 1,000,000 feet, according to circumstances.

Such a circular motion of a preponderating central mass, act-

* The dispositions to fly off in a tangent, and fall to the sun, given to the planets by the Newtonian philosophy, are gratuitous assumptions, which one almost blushes to name, and are unsupported by any analogy, and unwarranted by the universal simplicity of the machinery of nature.

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ing on and through the medium of space upon the planetary bodies, or upon any aggregations of matter, would propel them into corresponding motions, with forces varying reciprocally as the squares of the distances, and directly as the quantities of matter to be moved. *Hence the orbicular motions of the planets**.

If the result of this action were a balance of momenta in the moving bodies, as directly exerted and dissipated in the medium of space, then the orbicular motion would terminate the phænomena; but, if the continuous mass of the planetary body were unequally acted upon, owing to its sides being of different density, then the equal action of the prime mover would drive the lighter hemisphere round the heavier (as the Pacific Ocean round the old Continent); *and a rotary motion would necessarily be generated, whose axis would equalize the quantities of matter on each of its sides.*

Of course such an action, constantly exerted on various bodies distributed through space, would cause them to vary their respective motions, according to their positions in relation to each other; because the force on each would be as their mutual positions in regard to the sun.—*Hence the mutual disturbances of the uniform motions of the planets.*

The motion thus created in every mass would, from a like cause, occasion each to act on the other, in proportion to its bulk and quantity of matter. The earth and moon would be acted upon by the sun; but the earth would also act upon the moon, more than the moon upon the earth, in the proportion of their matter. The common action of the sun on both would occasion them of necessity to endeavour to turn on the centre of the quantity of motion generated by each.—*Hence the revolution of small masses round large ones.*

But, as the secondary planets would be governed chiefly and proximately by their primaries, and these would possess a power of varying the centre of motion by the motion of their fluids, which would, from that cause, rise in the parts presented to the secondary; so the secondary would not turn on the centre of its own mass, but its disposition to do so would be destroyed by the varying or accommodating energies of the primary.—*Hence the peculiar motions of a secondary planet, and the necessary connexion of those motions with the tides of the primary.*

Of course also, as the axis of each mass, or of the joint masses of primaries and secondaries, would be constantly turning round the physical axis or centre balancing their quantity of motion,

* If the velocities were as the forces exerted, and the momenta were as the matter compounded of the square of the velocities, then the quantities of motion at each end of the line of action would in theory be equal.

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and as the moving power in the sun would be constantly impelling that moving axis—*the centre of density of the single or conjoined masses would describe the orbit round the sun, and its variations would tend to vary the curve of the orbit.*

The diameter of the orbit, or the radius vector, would therefore be slightly and regularly varied by any arrangement within the planet which enlarged the distance between the centre of motion and the centre of matter, as a preponderance of water in one hemisphere, either from construction or the melting of congealed masses*. Whatever varies the rotation of the axis of motion (that is, of the mass,) round the axis of the real matter in a planet, would necessarily vary its rotary impulse, increase or diminish its centrifugal force, and give a variation to the length of the radius vector; *and hence the elliptical form of the orbits of the planets.*

The masses of each planet would be kept together, and accidental disturbances in the arrangement of the parts would be restored by the subjection of each part to the paramount motions of the whole, as proved in my previous essay.

The medium of space, whatever it may be, would thus be an acting cause of motion, like a current of the sea, and not a means of resistance, as has been mistakenly supposed.

There would be no occult principle of attraction or gravitation concerned in any part of the phænomena; but the whole would be a necessary result of the known laws of motion, at once subordinate, analogical, harmonious, and fit. The phænomena of the universe are the results of a system of motion producing motion; and of motion generated by motion. By this agency a stone is propelled to a planet by the motions of the planet—a planet is carried round the sun by the motions of the sun—a secondary is carried round a primary by the joint motions of the sun and primary—and the motions of the sun are, perhaps, caused by the motions of systems of suns—while the motions of those systems may again be caused by other superior motions! In short, all nature consists of a series of included motions produced by the motions of superior bodies and systems, till we ascend to the first term in the series—an inscrutable CAUSE OF CAUSES!

The general mathematical laws would be the same as those heretofore determined, though the results would be produced by different trains of reasoning. The data would however be more

* It seems to be a *necessary* fact, that the cause which varies the direction of motion, or the equal orbit of a planet, should exist within the planet itself, and grow out of accidents arising from its general motion.

precise

precise and analogical, and the deductions, therefore, be more satisfactory.

I infer, generally, that MOTION *is the primary and proximate cause of all phænomena; that* IT *operates in a descending series from the rotation of the sun round the fulcrum of the solar system, to the fall of an apple to the earth; that, as transferred through all nature from its source,* MOTION *serves as the efficient cause of every species of vitality, of every organic arrangement, and of all those accidents of body heretofore ascribed to gravitation; and, I venture further to suggest, as a theological deduction, quite as probable as the doctrine of the Newtonians, which ascribes their gravitating or projectile force to the immediate agency of the Deity, that* MOTION, *as a great secondary cause, may be regarded, in its uniform operation from the great to the small, as the hand of* OMNIPOTENCE; *while, as a principle of causation, it necessarily involves the attribute of* OMNIPRESENCE.

However heretical this theory may appear to partisans of "the gravitating principle," to believers in "gravitating particles," to devotees of "harmonic numbers," to geometricians who consider the laws of curves as laws in physics, or to philosophers who conceive that body may act without material intervention where it is not, I consign it to the guardianship of the press, in full confidence that it will surmount opposition, and endure as long as the system which it describes.

R. PHILLIPS.

XXXIX. *Notices respecting New Books.*

An Experimental Inquiry into the Laws of the Vital Functions; with some Observations on the Nature and Treatment of Internal Diseases. *By* A. P. WILSON PHILIP, *M.D. F.R.S.E.* 8vo. 360 pages.

IN our Number for May, we announced that this work was in the press. It has now made its appearance. It is divided into two parts. In Part I. the author treats of the state of our knowledge respecting the principle on which the action of the heart and blood-vessels depends, and the relation which subsists between them and the nervous system; giving a translation of the Report of the Committee of the National Institute of France, on the experiments of M. le Gallois, which he considers as accurate, well arranged, and sufficiently comprehensive for this purpose. In other respects, however, he considers this Report as not deserving the same praise, as "it overlooks defects both in M. Gallois'

Gallois' experiments and reasonings, of such moment, as wholly to invalidate all his most important conclusions; and to leave him the discoverer of certain unconnected though most valuable facts, instead of the author of a new system, founded, as the Report alleges, on a basis never to be shaken. In Part II. the author relates his own experiments, and points out the inferences to which they seem to lead—respecting the principle on which the action of the heart and vessels of circulation depends; the relation which subsists between these and the nervous system; the principle on which the action of the muscles of voluntary motion depends, and the relation which they bear to the nervous system; the principle on which the action of the vessels of secretion depends, and the relation which they bear to the nervous system; the nature of the nervous influence; the principle on which the action of the alimentary canal depends, and the relation which it bears to the nervous system; digestion, and the effects produced on the stomach and lungs by destroying certain portions of the spinal marrow, compared with those by dividing one or both of the eighth pair of nerves.

The author then proceeds to "the temperature of the animals in those experiments in which portions of the spinal marrow were destroyed," or, generally, "*the cause of animal temperature.*" Alluding to Mr. Brodie's Croonian lecture for 1810, in which he gave an account of experiments which led to the inference, that the production of animal temperature is under the influence of the nervous system; to the same gentleman's experiments in the Philosophical Transactions of 1812, tending to strengthen this inference; and to his own, which tend to prove that the caloric which supports animal temperature, is evolved by the same means, namely, the action of the nervous influence on the blood, by which the formation of the secreted fluids is effected, and consequently that it is to be regarded as a secretion—he observes that "if this view of the subject be correct, and galvanism be capable of performing the functions of the nervous influence, it ought to occasion an evolution of caloric, as it effects the formation of secreted fluids, from arterial blood, after the nervous influence is withdrawn." To ascertain this point, certain experiments were made on animals, which he details at length, and which, he suggests, "afford by their result a strong argument *in favour of the identity of the nervous influence and galvanism.*" He next considers the use of the ganglions; the relation which the different functions of the animal body bear to each other, and the order in which they cease in dying; reviews the inferences from his experiments and observations; and concludes with the application of these to explain the nature and improve the treatment of diseases.

The work before us deserves much attention from medical men. As a specimen of the author's style, and the way in which he applies the result of his inquiries to useful purposes, we select the following from the concluding part of his work: He says: "I cannot help regarding it as almost ascertained, that in those diseases in which the derangement is in the nervous power alone, where the sensorial functions are entire, and the vessels healthy, and merely the power of secretion, which seems immediately to depend on the nervous system, is in fault, galvanism will often prove a valuable means of relief."

"*Of Asthma and Dyspepsia.*

"The following observations relate chiefly to affections of the lungs. Of the effects of galvanism in dyspepsia, the principal experience which I have yet had, has been in cases where it was complicated with asthmatic breathing.

"When the effect of depriving the lungs of a considerable part of their nervous influence is carefully attended to, it will be found, I think, in all respects similar to a common disease, which may be called habitual asthma; in which the breathing is constantly oppressed, better and worse at different times, but never free, and often continues to get worse in defiance of every means we can employ, till the patient is permanently unfitted for all the active duties of life. The animal, in the above experiment, is not affected with the croaking noise and violent agitation which generally characterize fits of spasmodic asthma. This state we cannot induce artificially, except by means which lessen the aperture of the glottis.

"We have seen from repeated trials, that both the oppressed breathing and the collection of phlegm, caused by the division of the eighth pair of nerves, may be prevented by sending a stream of galvanism through the lungs*. That this may be done with safety in the human body we know from numberless instances, in which galvanism has been applied to it in every possible way.

"Such are the circumstances which led me to expect relief from galvanism in habitual asthma. It is because that expectation has not been disappointed, that I trouble the reader with the following account of its effects. Although the effects of galvanism in habitual asthma have been witnessed by many other medical men, I have mentioned nothing in the following pages which did not come under my own observation.

"I have employed galvanism in many cases of habitual asthma, and almost uniformly with relief. The time, during which the galvanism was applied before the patient said that his breathing

* Exp. 46, 47, 48, 49.

was

was easy, has varied from five minutes to a quarter of an hour. I speak of its application in as great a degree as the patient could bear without complaint. For this effect I generally found from eight to sixteen four-inch plates of zinc and copper, the fluid employed being one part of muriatic acid, and twenty of water, sufficient. Some require more than sixteen plates, and a few cannot bear so many as eight; for the sensibility of different individuals to galvanism is very different. It is curious, and not easily accounted for, that a considerable power, that perhaps of twenty-five or thirty plates, is often necessary on first applying the galvanism, in order to excite any sensation; yet after the sensation is once excited, the patient shall not perhaps, particularly at first, be able to bear more than six or eight plates. The stronger the sensation excited, the more speedy in general is the relief. I have known the breathing instantly relieved by a very strong power. I have generally made it a rule to begin with a very weak one, increasing it gradually at the patient's request, by moving one of the wires from one division of the trough to another, and moving it back again when he complained of the sensation being too strong. It is convenient for this purpose to charge with the fluid about thirty plates.

"The galvanism was applied in the following manner. Two thin plates of metal about two or three inches in diameter, dipped in water, were applied, one to the nape of the neck, the other to the pit of the stomach, or rather lower. The wires from the different ends of the trough* were brought into contact with these plates, and, as observed above, as great a galvanic power maintained, as the patient could bear without complaint. In this way the galvanic influence was sent through the lungs, as much as possible, in the direction of their nerves. It is proper, constantly to move the wires upon the metal plates, particularly the negative wire, otherwise the cuticle is injured in the places on which they rest. The relief seemed much the same, whether the positive wire was applied to the nape of the neck, or the pit of the stomach. The negative wire generally excites the strongest sensation. Some patients thought, that the relief was most speedy, when it was applied near the pit of the stomach.

"The galvanism was discontinued as soon as the patient said that his breathing was easy. In the first cases in which I used it, I sometimes prolonged its application for a quarter of an hour, or twenty minutes, after the patient said he was perfectly relieved, in the hope of preventing the early recurrence of the dyspnœa; but I did not find that it had this effect. It is remarkable, that in several who had laboured under asthmatic breathing for from ten

* I found a trough of the old construction answer better than the improved pile, which is so much superior for most purposes.

 to

to twenty years, it gave relief quite as readily as in more recent cases; which proves, that the habitual difficulty of breathing, even in the most protracted cases, is not to be ascribed to any permanent change having taken place in the lungs.

"With regard to that form of asthma which returns in violent paroxysms, with intervals of perfectly free breathing, I should expect little advantage from galvanism in it, because, as I have just observed, I found that the peculiar difficulty of breathing, which occurs in this species of asthma, cannot be induced in animals, except by means lessening the aperture of the glottis. It is probable, that in the human subject the cause producing this effect is spasm, from which indeed the disease takes its name, and we have no reason to believe, from what we know of the nature of galvanism, that it will be found the means of relaxing spasm."

[To be continued.]

Mr. Accum, author of several well-known works on Chemistry and Mineralogy, has just published a new work entitled "Chemical Amusement," comprising a series of curious and instructive experiments on chemistry, which are easily performed and unattended with danger. The work has been written, the author states, "with a view to blend chemical science with rational amusement. To the student they may serve as a set of popular instructions for performing a variety of curious and useful experiments well calculated for illustrating the most striking facts which the science of chemistry has to offer. To give effect to this object, the author has selected such experiments only as may be performed with ease and safety in the closet, and the exhibition of which requires neither costly apparatus nor complicated instruments. And that the experiments may be of greater value than merely to afford amusement for a leisure hour, he has added the explanation to each individual process, in order to enable the operator to contemplate the phænomena with advantage as particular objects of study, if his inclination should lead him that way."

The first number of a new periodical work, entitled "Journal of the Academy of Natural Sciences of Philadelphia," has just reached this country from America. It contains, 1st, Descriptions of six new species of the genus *Firola*, from the Mediterranean, by MM. de Sueur and Peron, with a plate. 2d, An account of the new mountain sheep, *Ovis montana*, by Mr. George Ord; with a wood engraving of the horn of the animal. 3d, A description of seven American water and land shells, by Mr. Thomas Say.

Another

Another Part (II. Vol. XI.) has appeared of The Edinburgh Encyclopædia, conducted by Dr. Brewster.

The principal articles are, Hybernation, Hydrodynamics, Hygrometry, Jamaica, Japan, Java, Ice, Iceland, Ichthyology, Idria, Jedburgh, Jersey, Jerusalem, Jesuits, and Jews.

In the article Hydrodynamics, the various subjects of Hydrostatics, Specific Gravities, Hydrometers, Equilibrium of Floating Bodies, Capillary Attraction and Cohesion, Hydrostatic Instruments, Hydraulics, Motion of Water in Tubes, Pipes, and Canals, Resistance of Fluids, and Hydraulic Machinery, are treated in a plain and popular manner, so as to be easily understood by those who have but a slight acquaintance with Mathematics.—Among the parts of this article which have never before appeared in our language, are an account of Laplace's Theory of Capillary Attraction; of Gay-Lussac's Instrument for measuring the Ascent of Water in Capillary Tubes; Venturi's Experiments on Floating Cylinders of Camphor; Girard's Experiments on the Effect of Heat upon the Motion of Fluids; and Prony's Researches on the Motion of Water in Pipes and Canals. The Experiments of Mr. Smeaton, on the Motion of Water in Pipes, are here printed, for the first time, from the MSS. of that celebrated Engineer, and the Description of some new Hydrometers, of Burns's Overshot Wheel without an Axle; of Burns's Sluice Governor; and of the Screw Engine, erected at the Hurlett Alum-works, have never before been published. New Tables for facilitating the application of Dubaut's Formulæ have also been computed for this article, by Mr. Lawrie of Glasgow. The article Hygrometry contains an account of the recent investigations of Gay-Lussac and Biot, and of many important discoveries made by Mr. Anderson of Perth, the author of the article, who has reduced into the form of a science a subject hitherto obscure and little understood.

The article Ice contains an account of the Observations of Mr. Scoresby on the Polar Ice; and the article Iceland is written by an eminent traveller, who lately visited that interesting island.

The Second Part of Lackington and Co's Catalogue, containing the Classes, curious and rare Books, old Plays, Astrology, Poetry, and the Arts, Philosophy, Natural History, Games and Sports, &c. &c. is now published. The Third Part, containing Greek and Latin Classics and Books in all foreign Languages, will be published in October; and the Fourth and last Part at Christmas, which will contain a very large Collection of Divinity, and an Appendix of additions to all the Classes.

Part the First, of English and Foreign History, Voyages, Travels, and Miscellaneous, is recently published.

 XL. In-

XL. *Intelligence and Miscellaneous Articles.*

CURIOUS COMPOUND OF PLATINUM.

Mr. Davy, Professor of Chemistry in the Cork Institution, whilst pursuing some investigations on platinum, formed a peculiar compound of this metal which has some remarkable properties. When it comes in contact with the vapour of alcohol at the common temperature of the air, there is an immediate chemical action, the platinum is reduced to the metallic state, and the heat produced is sufficient to ignite the metal and to continue it in a state of ignition. It would at present be premature to offer any conjectures on the uses to which this new compound may be applied; but from the peculiar properties both of the metal and the compound, there is reason to believe it will admit of some important applications. Mr. Davy has already employed it as a simple and easy means of affording heat and light. To produce heat, nothing more is necessary than to moisten any porous animal, vegetable, or mineral substance, as sponge, cotton, asbestos, iron filings, sand, &c. with alcohol or whiskey, and let a bit of the compound fall on the substance so moistened; it instantly becomes red hot, and continues to remain so whilst any spirit remains; nor is it extinguished by exposure to the atmosphere, or by blowing the breath on it; on the contrary, partial currents of air only make the ignited metal glow brighter. The heat produced in this way may be accumulated to a considerable extent by increasing the quantity of the materials employed. On these facts, Mr. Davy has constructed a sort of tinder box that answers very well to procure immediate light. The box contains two small phials, and some sulphur matches tipped at the points with a very minute bit of phosphorus; one of the phials contains the compound; the other a little alcohol. The phials may either have glass stopples or corks. The stopper of the phial containing the alcohol has a small aperture at the bottom, in which there is inserted a bit of sponge; this is kept moistened but not quite wet with alcohol. When a light is wanted, it is only necessary to take out the stopper and put a bit of the compound no bigger than the head of a pin on the moistened sponge; it instantly becomes red hot, and will immediately light one of the matches.

This mode of igniting a metal and keeping it in a constant state of ignition, is quite a novel fact in the history of Chemistry, and affords a happy illustration of the facts pointed out by Sir Humphry Davy in his late able and scientific researches, which have thrown so much light on the philosophy of flame, led to such brilliant and highly important results, and will probably admit us to a more intimate acquaintance with Nature in her refined and elaborate operations.

CHLORINE.

CHLORINE.

Dr. Ure of Glasgow has lately finished a very elaborate series of experiments on the controversial subject of chlorine. Their principal object was to ascertain whether water, or its elements, existed in and could be extracted from muriate of ammonia. He has perfectly succeeded in obtaining water from the dry and recently sublimed salt, by methods quite unexceptionable. The vapour of such muriate of ammonia being transmitted through laminæ of pure silver, copper and iron, ignited in glass tubes, water and hydrogen were copiously evolved, while the pure metals were converted into metallic muriates. This fact is decisive, in the Doctor's opinion, of the great chemical controversy relative to chlorine and muriatic acid, and seems clearly to establish the former theory of Berthollet and Lavoisier, in opposition to that more lately advanced by Sir H. Davy with such apparent cogency of argument as to have led almost all the chemists of Europe to embrace his opinion. The details of the experiments have been communicated some time since to a distinguished member of the Royal Society, and will be speedily laid before the public. This decomposition of the salt by the metals, at an elevated temperature, is analogous to the decomposition of potash in ignited gun-barrels, by Gay-Lussac and Thenard.

STEAM ENGINES IN CORNWALL.

It appears from Messrs. Lean's Report, that during the month of August 29 engines performed the following work with each bushel of coals.

		Water lifted 1 foot high with each bushel.	*Load per square inch in cylinder.*
21 common engines averaged		22,301,735	various.
Woolf's at Wheal Vor	..	37,031,002	15·5 lib.
Ditto Wh. Abraham	..	51,067,670	16·8
Ditto ditto .	..	25,841,894	4·2
Ditto Wh. Unity	..	29,417,746	13·1
Dalcouth engine ..	..	44,125,715	11·2
Wheal Abraham ditto	..	34,288,322	10·3
United Mines ditto ..	..	35,347,917	18·1
Wheal Chance ditto	..	34,489,691	10·7

SAFETY LAMP.

Sir Humphry Davy has made a further discovery in regard to combustion, which will prove a very great improvement to his safety lamp. He thus describes it in a letter to the Rev. J. Hodgson of Heworth:—

"I have succeeded in producing a light perfectly safe and œconomical, which is *most brilliant* in atmospheres in which the

 flame

flame of the safety-lamp is extinguished, and which burns in every mixture of carburetted hydrogen gas that is respirable. It consists of a slender metallic tissue of platinum, which is hung in the top of the interior of the common lamp of wire gauze, or in that of the twilled lamp. It costs from 6d. to 1s., and is imperishable. This tissue, when the common lamp is introduced into an explosive atmosphere, becomes red hot, and continues to burn the gas in contact with it as long as the air is respirable; when the atmosphere again becomes explosive, the *flame* is relighted. I can now burn any inflammable vapour either with or without flame at pleasure, and make the wire consume it either with red or white heat. I was led to this result by discovering slow combustions without flame, and at last I found a metal which made these harmless combustions visible."

TRIGONOMETRICAL SURVEY.

Dr. Olinthus Gregory and Colonel Mudge, who it will be recollected formed a part of the scientific association which lately proceeded to the Zetland Isles, have just returned. Captain Colby and M. Biot remain in Zetland a few weeks longer; the former for the purpose of terminating his observations with Ramsden's zenith sector, and then of connecting the chief points in the triangulation; the latter, in order to witness the phænomena of the Auroræ Boreales in these high latitudes Dr. Gregory having ascertained what is technically denominated "the *rate*" of Pennington's astronomical clock at Balta, in north latitude 60. 45, proposes staying a short time at Aberdeen, for the purpose of ascertaining the rate of the same clock there, by means of astronomical observations with the excellent instruments in the Observatory at Marischal College.

ERUPTION OF VESUVIUS.

A letter from Naples, dated July 20, says—"The present eruptions of Vesuvius are astonishing. Copper, iron, alkaline acid, sulphur, sulphuric acid, chalk, and ammonia, form salts that are sometimes in a mass and sometimes divided. It is observed that copper is very much mixed with volcanic matter; quantities of it are found among the different kinds of lava. Vesuvius, which since the year 1813 has been more or less in a state of commotion, has entirely covered its former crater with a thick crust, over which the new eruptions have thrown two little mountains, from which come smoke, ashes, and vitrified stones. The earth is covered with bits of transparent glass. This crust is so considerable, that, if it is not propped up, the sinking of the matter composing it will produce an effect like that of the eruption which took place in the time of Titus."

NAUTICAL

NAUTICAL ALMANACK.

To Mr. Tilloch.

Sir,—It is to be regretted that the omissions and erroneous figures and calculations still continue to be so numerous in the Nautical Almanack, to the great hazard and danger of our navigation and commerce. It would render that work in some degree more useful to the nation, if you would publish in your useful Magazine the following, being part of the errata in the Almanack for the year 1819.

Terricola.

Facing pa. 1, Moon's eclipse, April 10, middle 51 $\frac{2}{10}$ query 5 $\frac{0}{10}$.
Pa. 4. ☿ gr. elong. 31 days, for day.
——— ♀ passage merid. day 25, for 21 13, read 21 23.
— 18, last col. 1st day, add *N*.
——— 5 day, right ascens. midnight, for 88 44, read 89 44.
— 28, 1 day, ♃ declin. after 19 24 set *S*.
— 37, 28 day, for Easter T. *ends*, set Easter T. *begins*.
— 38, days 12, 13, 14, 15, equat. of time, for 1, 1, 1, 1, set 0, 0, 0, 0.
— 40, ☿ helioc. long. 13 day, for 79, set 19.
— 42, 9 day, declin. noon, after 1 10 set *S*.
— 49, 17 day in the Calendar, set *Prs. of Wales born*.
— 50, 16 day, declin. for 85, set 58.
— 52, ☿ gr. elong. for days read day.
——— 13 day, last col. for 12 59, set 22 59.
——— ♃ 1 day, declin. after 16 49, set *S*.
——— last col. for 18 13, set 18 39.
——— 13 day, last col. for 17 56, set 17 55.
— 54, 19 day, passage merid. for 22 12, set 21 12.
— 61, 10 day, in the Calendar, set *Corpus Christi*.
— 62, 5 day, col. equat. of time, for 1 2, 2, set 2 2, 2.
— 64, ♂, 19 day, declin. for 11 59, set 12 59.
— 66, 27 day, passag. merid. for 5 52, set 3 52.
— 76, last line, helioc. long. read 8 22 56.
— 85, in the Calendar, 7 day, dele *Prs. Amelia b.*
———————, 11 day, dele *Ds. of Bruns. b.*
— 86, 21 day, col. equat. of time, for 3 9, 9, set 3 3, 9.
— 88, ☿, 28 day, geocen. long. for 5 27, set 5 21.
———, ♃, 25 day, last col. for 10 54, set 10 34.
— 100, ☿, 16 day, helioc. long. for 2 0 30, set 1 0 30.
———, ♃, geocen. long. 7 day, for 9 33, set 8 31.
———————, 13 day, for 8 3, set 8 5.
———————, 19 day, for 7 43, set 7 44.
———, ♄, helioc. long. 1 day, for 26 90, set 26 40.
———, ♅, last col. 21 day, for 6 27, set 5 27.
— 102, 12 day, rt. asc. midn. for 96 25, set 98 25.
———, 30 day, declin. noon, for 27 14, set 17 14.

Pa. 112,

Pa. 112, ♀, geoc. long. 1 day, for 4 17, set 5 17.
————————————, 19 day, for 27 57, set 27 47.
————♂————, 13 day, for 20 23, set 3 20 23.
— 113, long. noon, 27 day, for 10 15, set 10 19.
— 114, passage merid. 3 day, for 11 18, set 12 18.
— 121, 3 day, set *Prs. Sophia b.*
— 133, full moon, for 1 0 11, set 1 6 11.
— 138, rt. ascens. midn. 11 day, for 191 85, set 191 55.

LECTURES.

The following arrangements have been made for Lectures at the Surry Institution during the ensuing Season :—

1. On Ethics, by the Rev. W. B. Collyer, D.D. F.S.A. To commence on Tuesday, Nov. 4, at Seven o'clock in the evening, and to be continued on each succeeding Tuesday.

2. On Chemistry, by James Lowe Wheeler, esq. To commence on Friday, Nov. 7, and to be continued on each succeeding Friday evening at the same hour.

3. On the British Poets, from Chaucer to Cowper, by Wm. Hazlitt, Esq. To commence early in Jan. 1818.

4. On Music, by Wm. Crotch, Mus. D. Professor of Music in the University of Oxford. To commence early in Feb. 1818.

Mr. T. J. Pettigrew, F.L.S. Surgeon Extraordinary to their Royal Highnesses the Dukes of Kent and Sussex, will commence his Winter Course of Lectures on Anatomy, Physiology, and Pathology, on Friday the 17th of October, at Eight o'clock in the evening precisely. The Lectures will be continued every succeeding Wednesday and Friday at the same hour, until completed. Particulars may be known by applying to Mr. P., Bolt Court, Fleet-street.

Dr. Clutterbuck will begin his Autumn Course of Lectures on the Theory and Practice of Physic, Materia Medica, and Chemistry, on Friday, Oct. 3d, at Ten o'clock in the morning, at his house, No. 1, in the Crescent, New Bridge Street, where further particulars may be had.

Pupils are admitted as usual, to attend the medical practice of the Dispensary, and, when qualified, to visit the patients at home. Clinical Lectures on the most interesting and instructive cases that occur, will be given weekly by the Physicians in rotation.

The Lectures on Midwifery at the Middlesex Hospital, by Dr. Merriman, Physician-Accoucheur to that Hospital, and Dr. Ley, Physician-Accoucheur to the Westminster Lying-in Hospital, will recommence as usual early in October.

Mr. Clarke will commence his Lectures on Midwifery and the Diseases of Women and Children, on Friday, October 10th. The Lectures are read every morning from a quarter past Ten to a

quarter

quarter past Eleven, for the convenience of Students attending the Hospitals. For particulars apply to Mr. Clarke at the Lecture Room, 10, Saville Row, Burlington Gardens.

Mr. Guthrie, Deputy Inspector of Military Hospitals, will commence his Autumn Course of Lectures on Surgery, on Monday the 6th of October, at Five minutes past Eight in the evening, in the Waiting-room of the Royal Westminster Infirmary for Diseases of the Eye, Mary-le-bone Street, Piccadilly. To be continued on Mondays, Wednesdays, and Fridays.

Two Courses will be delivered during the Season.

In each Course the Principles of Surgery will be explained, and the practice resulting from them, with reference both to Domestic and Military Surgery, fully pointed out.

The Operations referred to in the Lectures will be shown in each Course.

Terms of Attendance.—Perpetual Five guineas. Single Course Three guineas.

Medical Officers of the Navy, the Army, and the Ordnance, will be admitted gratis, on obtaining a recommendation from the Heads of their respective departments, which must be presented to Mr. Guthrie between the hours of Two and half-past Four, at his House, No. 2, Berkeley Street, Berkeley Square.

Mr. Gaulter will deliver in the ensuing Season, two Courses of Lectures on the Physiology of the Human Body, at No. 10, Frith-street, Soho-square. The Lectures will be given on Monday and Thursday Evenings at a Quarter past Eight o'clock, after the Surgical Lectures are concluded. The Introductory Lecture of the first Course will be on Thursday the 9th of October.

*** In last Number we stated that the Course of Lectures at Guy's Hospital, on the Structure and Diseases of the Teeth, was to be delivered by Mr. Fox; instead of which it should have been by Mr. Thomas Bell, who has been appointed to succeed Mr. Fox.

LIST OF PATENTS FOR NEW INVENTIONS.

To John Perks, of Carey-street, St. John's, Westminster, Middlesex, for certain improvements in the apparatus for manufacturing, purifying and storing gas.—Dated 5th August 1817.—6 months allowed to enroll specification.

To Thomas Taft, of Birmingham, for an improvement in bridle and other reins used and affixed to bitts and leather sliding loop to act with reins and bitts.—5th August.—6 months

To Samuel Mersey the younger, of Long-Acre, Middlesex, for his improved mode or method of weaving, making, and manufacturing of livery lace and coach lace.—7th August.—2 months.

ASTRO-

ASTRONOMICAL PHÆNOMENA, OCTOBER 1817.

D. H. M.		D. H. M.	
1.16.15	I } of 125 ♉ * 5′ S.	13. 1.12	☽ δ ♍
1.17.33	E } of ☽'s cent.	13.15.41	☽ ♃
3.19.41	☽ υ ♊	14. 8 8	☽ θ Ophiuchi
4.10.41	☽ η ♌	15.16 30	☽ φ ♐
8.11. 5	☽ 3ν ♍	15.19 43	☽ σ ♐
9.13.11	☽ γ ♍	18.15.23	☽ ε ♑
10. 2 10	☽ θ ♍	23. 6 34	☉ enters ♍
11. 8. 4	☽ λ ♍	24. 2.30	☽ 110 ♓
11.21. 3	☽ α ♎	26. 0. 0	☽ in apogee
12. 0. 0	☽ in perigee	29. 0. 6	☽ 125 ♉
12.21.50	☽ λ ♎	31. 3. 6	☽ υ ♊

The eclipse of Jupiter's 3d satellite on the 23d, which is set down in the Nautical Almanack, as visible at Greenwich, will not be visible. Indeed the only one that is so, is the emersion of the first satellite on the 29th, which is not marked in the Almanack. The emersion of the third on the 9th at $1^h\ 17^m\ 24^s$ ought to be $2^h\ 17^m\ 24^s$

METEOROLOGY.

Sun's Atmosphere.

[From the *Political Zeitung* of Munich, of the 10th August.]

"The great and remarkable opening in the sun's atmosphere of clouds (*wolkigen sonnenatmosphare*), of which notice has been lately taken, was visible only a little before it vanished at the western edge on the 5th of August, at which period a number of little openings began to unite themselves into two spots; storms and much rain followed. It must be of great utility to farmers to be able to foretel fair or stormy weather, from observations of the spots on the sun, which are easily examined in the middle of summer, in the same way as we can do for the coming day or night, by the rising and setting sun. A great number of the latest observations confirm Herschel's opinion, that like the planets (*verander lichen sternen*) one half of the sun is less favourable to an abundant discharge of rays than the other, and that many spots on the sun make the year warmer and more fruitful. So much is certain, that in defect of spots on the sun, the atmosphere is more serene, as happened in the year 1811, in which none appeared during the whole summer; but it showed likewise that such a year must not of necessity be unfruitful, as was the case in the years 1795 and 1799. It is yet more certain that very warm and very cold weather can alone depend on the periodical abundance or scarcity of combustible matter (*brennstoff*) in the sun, since the moon and the planets can neither cause heat nor cold. In the year 541, which was one of famine and pestilence, the rays of the sun, according to Cedrenus, were as feeble as those of the moon, and yet the weather was so clear that in Italy they

observed

observed the comets of that time; the chronicle writers remark, that excessively dry summers (as the year 763, and the year 1800, remarkable for spots on the sun, and woods taking fire,) follow a very copious appearance of meteors (*sternschnuppen*). In nature great matters more constantly depend upon each other than minute, and it becomes us to observe and take advantage of that dependance: it is to be wished therefore, that meteorologists may apply themselves to a diligent observation of the spots on the sun."

Meteorological Observations kept at Walthamstow, Essex, from August 15 *to September* 15, 1817.

[Usually between the Hours of Seven and Nine A.M. and the Thermometer (a second time) between *Noon* and Two P.M.]

Date.	Therm.	Barom.	Wind.
August			
15	60 66	29·66	S—SW.—Sunshine; slight showers, and sun, and windy; fine day; bright star-light.
16	58 70	29·90	SW—SE.—Sun and hazy; cloudy and windy; fine afternoon; showers after 5 P.M.
17	55 62	29·65	W—SW.—Sun and clouds, and wind; showers and sun; bright star-light.
18	55 64	29·98	W—SW.—*Cirrostratus*, and calm; cloudy; showers after 3 P.M.; damp and hazy; much rain in the last night.
19	61 68	29·76	SW—SW.—Cloudy and hazy; showery day; cloudy. Moon first quarter.
20	55 78	29·75	SW.—Clear and *cumuli*; fine day; moon- and star-light.
21	55 62	29·78	N.—Wind and rain; great showers; stars and *cirrostratus*.
22	46 65	30·10	NW—NE.—Clear and windy; clear and *cumuli*; very fine day; moon through *cirrostratus*.
23	42 63	30·10	E.—Clear and windy; clear and *cumuli*; very fine day; clear moon- and star-light.
24	60 61	29·77	E.—Cloudy; showery about noon; fine afterwards; a shower at 9 P.M.
25	57 58	29·32	E—SE.—Gray; rainy after 7 A.M.; showers all day; clear star-light.
26	53 63	29·10	E—SE.—Fine clear morn; great showers, and sun; stars, and *cirrostratus*. Full moon.
27	51 68	29·00	SE—SW—W—NW.—Clear and *cirrostratus*; fine day; very slight showers; stars and *cumuli*.
28	52 67	29·66	NW—SW.—Fine, sun, and wind; very fine day; no rain this day; cloudy, but light.

August

August			
29	52	29·66	W.—Clear, and *cirrostratus;* fine day; slight showers, and sun; clear moon and star-light.
	67		
30	53	29·87	SW—W.—Very fine morn; cloudy, and dark; some rain in the evening.
	71		
31	58	29·99	SW.—Sun and wind; fine day; windy; no rain today; bright star-light.
	66		
September.			
1	48	29·99	SE—SE.—Fine morning; calm; fine day; hot; no rain; star-light.
	65		
2	46	30·00	NE—SE.—Sun and hazy; white dew; very fine day; dark night at 9 P.M.; star-light late.
	69		
3	59	29·97	SE.—Very hot fine morning; fine day; star-light. Moon last quarter.
	74		
4	59	30·00	N by W.—*Cirrostratus,* and very hot; fine day; hot and windy; bright star-light.
	68		
5	50	30·20	N—W—SW.—Very fine hot morning; fine day; star-light.
	72		
6	50	30·20	NE—SW—E.—Hazy, and sunny; fine hot day; star-light.
	72		
7	68	30·20	SE—E.—Foggy; deep azure sky, and *cumuli* at 11 A.M.; hot sunny day; 6½ P.M. orange sunset, and purple mottled *cirrostratus;* star-light.
	59		
8	51	30·00	NW—NE—SW.—Foggy; fine hot day; star-light and wind.
	76		
9	57	29·00	N—NW.—Hazy, and wind; fine day; dark night.
	67		
10	57	30·10	E—N.—Gray morning; sun after 1 P.M.; fine day; star-light.
	66		
11	53	30·09	N—E—E by S—SW.—Rather hazy; sun after 3 P.M.; fine day; bright star-light. New moon.
	64		
12	55	30·00	S.—Hazy; a shower at 8 A.M.; sun; clouds and wind; fine day; star-light.
	68		
13	62	29·98	NE—E.—Gray, and *cirrostratus;* gray day, and slight showers; dark night.
	64		
14	55	29·98	NE—SE.—Rainy till near 11 A.M.; *cirrostratus;* stars and clouds.
	59		
15	61	29·98	NE—SE.—Hazy, and very damp; cloudy day; cloudy.
	66		

At Tunbridge Wells, the 6th of August, a large and very brilliant meteor was seen; a slowly descending body of fire, which appeared about half the size of the moon's disk, and was highly coloured.

METEORO-

METEOROLOGICAL JOURNAL KEPT AT BOSTON, LINCOLNSHIRE.

[The time of observation, unless otherwise stated, is at 1 P.M.]

1817.	Age of the Moon.	Thermometer.	Barometer.	State of the Weather and Modification of the Clouds.
	DAYS.			
Aug. 15	2	68·	29·83	Fair—blows hard from the S.
16	3	67·	29·91	Fair—heavy rain at night
17	4	58·	29·76	Showery—thunder
18	5	64·	30·01	Fair—heavy rain at night
19	6	70·	29·80	Ditto—rain at night
20	7	68·	29·74	Ditto ditto
21	8	57·5	30·	Showery
22	9	61·	30·21	Fine
23	10	65·	30·10	Ditto
24	11	62·5	29·89	Cloudy
25	12	59·	29·29	Rain
26	full	64·5	29·20	Showery
27	14	61·	29·40	Ditto
28	15	63·	29·79	Fair
29	16	62·	29·73	Cloudy—heavy rain towards morn.
30	17	66·	30·	Fair
31	18	67·	29·96	Ditto—heavy rain towards morn.
Sept. 1	19	63·	30·18	Fine
2	20	65·	30·21	Very fine
3	21	72·	30·05	Ditto
4	22	68·	30·21	Ditto
5	23	70·	30·33	Ditto
6	24	72·	30·26	Ditto
7	25	71·	30·26	Ditto
8	26	71·	30·13	Ditto
9	27	62·	30·24	Cloudy
10	28	61·	30·16	Ditto
11	new	67·	30·19	Very fine
12	1	61·	30·01	Cloudy
13	2	61·	30·15	Very fine
14	3	55·	30·12	Cloudy—rain in the evening

This morning (Sept. 15) it rains again. The Barometer is however higher.

METEORO-

METEOROLOGICAL TABLE,

By Mr. Cary, of the Strand,

For September 1817.

Days of Month.	Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock, Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
Aug. 27	56	60	55	29.30	27	Stormy
28	56	[illegible]	57	·63	44	Fair
29	57	68	56	·64	46	Showery
30	57	65	60	·85	48	Cloudy
31	59	69	56	·80	58	Fair
Sept. 1	55	69	55	30·00	69	Fair
2	54	67	60	29·95	46	Fair
3	60	74	66	·84	42	Fair
4	60	71	60	30·02	78	Fair
5	55	69	59	·12	57	Fair
6	55	73	60	·[illegible]	67	Fair
7	56	70	60	·10	41	Fair
8	56	73	61	29·95	42	Fair
9	59	67	56	30·01	21	Fair
10	56	65	57	29·95	42	Fair
11	56	64	56	30·01	21	Fair
12	55	67	56	29·96	35	Fair
13	54	64	55	·94	25	Cloudy
14	54	61	60	·87	0	Rain
15	60	65	62	30·01	21	Cloudy
16	64	64	55	·01	24	Cloudy
17	55	66	60	29·90	36	Fair
18	56	60	58	·72	0	Rain
19	58	65	56	·84	32	Fair
20	55	64	57	30·00	30	Fair
21	54	60	52	29·95	27	Fair
22	48	60	54	·80	32	Fair
23	51	66	56	·82	36	Fair
24	55	61	55	·81	44	Fair
25	60	63	58	·52	35	Fair
26	57	60	50	·22	40	Stormy

N.B. The Barometer's height is taken at one o'clock.

XLI. *On Colours.—In Answer to Mr.* T. Hargreaves's *Strictures on the Work entitled "Chromatics; or, An Essay on the Analogy and Harmony of Colours."* *By* The Author.

To Mr. Tilloch.

Sir, — In answer to the observations of Mr. T. Hargreaves on my Essay entitled "Chromatics," &c. in your last Number, I beg to state that the pigments chosen to illustrate the various denominations of colours therein, have been selected from the most eminent for durability and beauty, and that I am not acquainted with any blue, red, or yellow, superior in these respects to the three pigments, *ultramarine*, *rubiate* or madder red, and *Indian yellow*, used in exemplification of the *primary colours*.

An eye critically nice will discern in every colour a tendency to some other colour, according as it is influenced by light, shade, depth or diluteness; nor is this the case only in the inherent colours of pigments, &c, but it is so also in the transient colours of the prism, &c. Hence blue in its depth inclines to purple; deep-yellow to orange, &c.; nor is it practicable to realize these colours to the satisfaction of the critical eye,—since perfect colours, like perfect geometrical figures, are pure ideals. My examples of colours are therefore quite as adequate to their office of illustrating and distinguishing, as the figure of an angle inclining to the acute or obtuse, instead of a perfect right angle, or middle form, would be in illustrating the conception of an *angle in general*.

Mr. H.'s objections to the examples of secondary and tertiary colours rest upon similar ground. Thus purple, composed of blue and red, (which in its perfect hue should neutralize or extinguish a perfect yellow,) denotes, in the example referred to, *not any particular or individual tint, but a class of tints, bounded on the one extreme by blue, and on the other by red:* and thus also of the other colours. The *secondaries*, purple, green, and orange, have accordingly been exemplified by intermediate tints composed of two of the primaries alternately; and the *tertiaries*, russet, citrine, and olive, by like intermediates of these secondaries; for all these denominations of colours, as above instanced, are indications of classes or genera, and not significant of invariable hues or tints of colour.

The remarks of Mr. H. are however perfectly just with respect to Example X. of the Essay, in which the neutralizing colours are contrasted, and consequently require such individuality of the opposed tints as may render them reciprocally neutralizing.

The foregoing remarks upon the particular relations of colours

 apply

apply equally to their general relations or harmonies *: for the harmonies are as infinite as the hues of colours, and no more is designed in the Essay than to generalize or class the harmonies: —the examples given of them, therefore, like the former, are only indications of instances of classes.

Mr. H. observes that the examples of the secondary colours are inferior in brilliancy to those of their primaries: but it is a principle in painting, to the value of which our great colourist, Sir Joshua Reynolds, has borne testimony, *that the compounds of colours are inferior in brilliancy, &c. to their components;* because pigments, being imperfect in hue, have a neutralizing or lowering effect upon each other, and a chemical action by which they are in general mutually injurious.

With respect to the denominations of the tertiary colours, I have already remarked that those I have adopted do not express individual hues or tints, but genera or classes; and since the nomenclature of colours, in all languages, is confessedly imperfect †, and I do not contend for tints or terms, I shall gladly change them for more significant appellations, if such can be found; but that I am not in error as to the thing signified, is manifest from § 16, in which it is remarked "that blue predominates in, and gives its relations to, the *olive*, yellow to the *citrine*, and red to the *russet*."

The use I have made of the double triangle in illustration of the relations of harmony in colours, in coincidence with Mr. H. is remarkable: yet, indeed, any trine figure might have supplied its place, though I have long preferred it for its simplicity, and as best suited to the philosophy upon which the Essay itself is founded.

To conclude. I am pleased to find that my system of colours, in respect to their *particular relations*, accords with the preconceptions of one so well acquainted with the subject as your correspondent appears to be; and since my doctrine of Harmony in Colours springs as a consequence from the same premises, and accords with the first principles of music, I anticipate, without desiring to bias his judgement, a like concurrence of ideas with that part of my Essay which treats of the *general relations or harmony of colours*.

I am, sir,

Yours very respectfully,

September 17, 1817. THE AUTHOR.

* See some excellent observations relating to this subject by Mr. Tredgold. Phil Mag. vol. xlix. p. 262.

† See Phil. Mag. vol. xlix. p. 49, On the Ancient Names of Colours, by T. Forster, esq.

XLII. Re-

XLII. *Report of the Select Committee appointed to consider of the Means of preventing the Mischief of Explosion from happening on board Steam-Boats, to the Danger or Destruction of His Majesty's Subjects on board such Boats.*

[Continued from p. 182.]

Mr. George Dodd's *Evidence.*

Where is your residence?—I reside at No. 8, Oxford-street.

What is your profession?—Civil engineer.

Are you a proprietor of any steam-boats?—I have five under my direction.

Where are those steam-boats employed?—Two between London and Richmond, one between London and Gravesend, and two between London and Margate.

How long have they, or any of them been in use?—The Thames has been in use three years.

Where does that go?—From London to Margate: the Majestic has been in use about twelve months, that goes to and from Margate: the Richmond, from London to Richmond, has been in use about fifteen months; and the other two are new vessels; all these vessels lie up in the winter. The Thames has not run from London to Margate during the whole three years; she has run from London to Margate two years, and was twelve months in Scotland before I had her. I finished the Thames Margate steam yacht at Port Glasgow in Scotland, and navigated her from Scotland to Dublin, and encountered a considerable deal of bad weather, and found her most perfectly safe. No material accident happened to the engine, which worked during the whole voyage; from Dublin I brought her round the Land's End, Cornwall, into the port of London.

Are all the steam-boats that you now have, or that you have had, used with condensing engines?—They are.

Has any accident happened during the course of their being used?—The boilers of two have been injured by the imprudence of the engine workers; but no accident of any description could or has occurred to the passengers.

What was the nature of the accidents that happened to those boilers?—The accident was the partial coming down of the boilers over the furnace mouth, being pressed down by the power of the steam, in consequence of the engine workers not sufficiently feeding the boilers, and covering the flues with water.

What are all your boilers made of?—They are made of sheet wrought iron, riveted together.

Are they cylindrical?—They are not; they are flat-sided with flat roofs, and the others have dome roofs; there are at least 1500

rivets in the larger ones; and I consider every rivet to be in a degree a safety-valve, as in all instances of bursting or tearing of this description of boilers the rivets first give way, and always give sufficient warning.

How many safety-valves have you to your boilers?—One to each.

Is that safety-valve accessible to the engineer directing the engine?—It is in all of them excepting the Richmond, and there it is under lock and key; the safety-valve on board of the Richmond is not a lever safety-valve, but they are simple weights resting on the safety-valve, the whole of which is inclosed within a box and locked up, so that no discretionary power is left to the man who works the engine; I carry the key of it in general myself.

Do not you think in future it would be advisable, in order for the greater safety of passengers, that boilers should be provided with two safety-valves, one not accessible to the engineer directing the engine, and another accessible to him?—I think that to all boilers there should be two safety valves; the one which would be accessible to the engine-worker, should be loaded with the minimum of the pressure that the chief engineer saw fit that the boiler should sustain; and that the other which would be inaccessible and locked up, should be loaded equal to the ultimatum that he would, under any circumstances, permit the boiler to support.

In a high pressure engine, what is your opinion of the weight that ought to be placed upon the safety-valve of its boiler?—That in a great measure is conjectural; but for my own practice, I certainly should not allow the safety-valves to be loaded with more than half the weight which I had previously tried and found the boiler was capable of supporting; all my engines are low pressure engines, and the weight upon the safety-valves is regulated not to exceed six pounds upon the inch.

What is the reason that you have adopted, in your steam-boats, the construction of boilers with flat sides and ends?—Because that figure gives the greatest cubical content in the smallest space, and compactness of the machinery and the boiler is a desirable object in a steam-boat.

Is it your opinion, that such boilers properly constructed, and of sufficient thickness in the plates of wrought iron, may be safely used on board steam-boats having the low pressure engines?—Most decidedly so; I consider each of my boilers capable of sustaining a pressure of fifteen pounds upon the inch, but I never work them to more than six pounds upon the inch.

Are those boilers so constructed, that the water entirely covers the tube in which the fire is made?—In the Richmond, the fire is entirely surrounded by the water; it is the case also in the Majestic; but in the Thames and in the new boat to Richmond,

and

and the new boat to Gravesend, they are what we call open furnace mouths: under the furnace mouth I place an ash-hole of cast iron, bedded in clay and upon fire-bricks.

Does the water in the boilers of this latter construction come to the upper surface of every portion of that iron, the under surface of which is exposed to the fire?—It does.

If you are acquainted with any accidents which have happened to steam-engines, not under your own direction, be pleased to mention what they were and how they happened?—I recollect the boiler of the Caledonia London and Margate steam-packet bursting at sea, by the forcing out of three of the rivets over the furnace mouth, which extinguished the fire, but it was not productive of any injurious consequences to any of the persons on board; and the Cork and Cove packet-boat in Ireland, with 250 officers and soldiers on board, burst her boiler when lying alongside of the transport that was receiving the troops; the bursting made a fissure or opening of nine inches by eighteen inches; but the steam which escaped did no injury either to the persons on board or to the vessel, nor do I think under any circumstances of the bursting of a wrought-iron boiler at the low pressure, that is, the steam not being more than ten or fifteen pounds to the inch, that the steam which might be suddenly let loose or disengaged, would have power sufficient to raise the deck of the vessel, or to injure the parties on board.

Supposing an engine upon the high pressure principle to have its boiler made of wrought iron, with the furnace passing through water throughout its whole length, and the boiler to be provided with safety-valves properly adjusted, so as to prevent the steam being raised to more than half of that pressure which the boiler is calculated to sustain, should you then have any apprehension of ill effects arising from the use of such an engine?—Certainly, I should still consider them hazardous and liable to very fatal consequences; for all boilers deteriorate by work, by time, and by oxidation, and what might be proof at this period, at a future period the boiler might be incapable of sustaining. Besides, all boilers are liable to casualties, and in case of any accident which might suddenly let loose or disengage the steam of a high pressure boiler, the steam itself would have sufficient expansive force and impetus to destroy any vessel. I have known instances, as I have stated before, of low pressure engines bursting, where they have done no injury; but I cannot conceive it possible that steam of ten or twenty times greater force could be let loose into the engine-room without creating mischief.

What is the average price of steam-boats calculated to convey passengers?—The Richmond steam-yacht cost, in the first instance, including the engine, 1800*l.* the engine itself cost about

 1000*l.*;

1000*l.*; the Majestic cost about 2000*l.* and the engine about 2000*l.* more; the Thames cost 2500*l.* including the engine, at about 1200*l.*; the new vessel that I built to go to Richmond, the hull and joiners' work cost 750*l.* and an engine of fourteen-horse power and apparatus cost 1170*l.*; the new Gravesend steam-yacht, the hull only has cost 750*l.* and the engine, 1370*l.*; but there will be various other expenses before these vessels are finished.

Can you tell what is the expense of the boiler alone?—I have just got a new boiler from Messrs. Jessops of Butterley, for the Thames steam-yacht, and I pay for the boiler 215*l.*

What additional expense do you apprehend is incurred in a boiler of these dimensions by having it of wrought iron, beyond what it would cost if made of cast metal?—Never having had any cast-iron boilers, I do not feel myself competent to give a satisfactory answer.

What additional expense would be incurred by the addition of an additional safety-valve?—That would depend upon the dimensions of the safety-valve, but in general the additional expense would be under 4*l.*

You mean that each safety-valve costs about that sum?—The most costly of them cost about that sum.

Did you ever apply a mercurial tube as a safety-valve?—Never: I have to each of the boilers a mercurial barometer, that operates as an indicator of the height and pressure of the steam.

Whereabouts is the expense of that barometer?—I do not recollect, but certainly not more than 2*l.*

Did you see the Norwich steam-packet which exploded?—I have been on board her, and performed a voyage with her; I went down with a view of purchasing it; I went down for that purpose twice.

What was your reason for not purchasing it?—Because it was a high pressure engine, and liable to the accident which has since occurred.

Was that your sole reason?—Yes; I went a second time with a party of German gentlemen from Bremen, who were anxious to make an immediate purchase of a steam-vessel; and they also declined to purchase that or any of the boats upon the river Yare, solely because they had high pressure steam-engines on board.

Did you examine the boiler which exploded when you were on board the vessel at Norwich?—I did.

What opinion did you form respecting that boiler?—I thought that it was injudiciously composed, as I found that the barrel or cylinder of the boiler was of wrought iron riveted together; of that part I approved, but I found that one of the ends was a flat plate of cast iron, and as these two metals under the same

degree

degree of heat have different degrees of expansion, I thought it by no means a perfect and secure boiler.

Had you any opportunity of observing the boiler, so as to form a judgement whether the cast-iron end was of sufficient strength to resist the pressure of the steam?—I had no such opportunity.

Had you any opportunity of observing when you were aboard, whether the steam was properly regulated?—Yes; I found that the safety-valve was pressed down by a lever, and when I first went on board, the steam was so high as to require the weight near the extreme end of the lever. My opinion respecting the insecurity of high pressure engines is not formed in consequence of the late accident; for on the 3d of March last, having occasion to write to a Mr. Rawlinson, who had applied to me to construct a steam-packet for his friends, I concluded my letter with these words; "Is it intended to have a low or a high pressure engine? if the latter, I should decline having any concern in the business, as they are attended with danger in any situation, but especially so in a steam-packet, where the lives of all on board would be at the mercy of the sobriety and attention of the engine worker."

You mean of course to say that they would be so if no precautions other than those which have hitherto been in use were adopted to prevent it?—Certainly; I allude to high pressure engines, as they have been hitherto usually arranged.

[Mr. George Dodd was again called in on a future day, and examined.]

Can you inform the Committee, or give them any general idea, what amount of capital is vested in steam-boats?—I have been on board and am well acquainted with twenty; and know that there are more than forty in Great Britain; many have cost 5000*l.* others 6000*l.* and one on the Thames above 10,000*l.*; I consider a fair average to be 3500*l.* each, making the vested capital 140,000*l.* Most of them are fitted up with peculiar elegance and accommodation, and the furniture and decorations alone form an expensive item; they are also very expensive to maintain, especially on the Thames, by reason of the great cost of coal. They are most numerous on the Clyde, where they have been productive of essential benefit to the general commerce and traffic of Glasgow, Port Glasgow, Greenock, and the neighbouring country.

What description of engines and boilers have the steam-boats, you personally know, or with which you are personally acquainted?—All I know have low pressure condensing engines, and wrought sheet-iron riveted boilers, except the remaining steam-boats between Yarmouth and Norwich, and one in Holland, built at Yarmouth; and they are high pressure engines.

[Mr. George Dodd was again called in and examined.]

For what purpose do you attend?—To produce a new safety-valve.

What are the advantages attendant on the safety-valve which you have to offer to the Committee?—I propose to the Committee the valve I now offer as a second valve, as it admits of being locked up so as to be inaccessible to the engine-worker; it prevents the possibility of his obstructing its action, either by going into the boiler when the boiler is cool, or under any circumstances whatever.—[*The witness produced it.*]

Is there any provision against the valve adhering in any part, so as to prevent its operation?—There is; the safety-valve has not a conical bottom as is usual in most safety-valves, but has a flat bottom resting upon a flat circular ring; the steam escapes from the sides of the box through apertures so constructed as that nothing can be introduced to impede its action.

Mr. Richard Wright's *Evidence.*

Where do you live?—At No. 62, Blackfriars Road.

You are an engineer?—Yes.

Do you know the cause of the explosion of the Norwich steam-boat?—I do not know it beyond this; that I know that the pressure must have been more than seventy-five pounds, having seen it worked at that pressure. My supposition is, that the man must have had it a vast deal beyond that, for there was no appearance of the boiler giving way at that time, and it was only a short time previous to the explosion itself.

Has anybody informed you, that to their knowledge the safety-valve of the engine was on that day, or on any other day, improperly loaded?—No; but they were frequently in the habit of putting an additional weight on the valve; this man in particular, in both the boats which he had occasionally worked.

Do you know any thing respecting the construction of the boiler?—The boiler was eight feet long; a cylindrical boiler four feet two inches diameter; it was first made with an internal angle iron at one end, and an external angle iron at the other end. In consequence of the internal angle iron having given way, a cast-iron end was substituted, which certainly was not done in a manner which I should have recommended; it might have been made safe certainly; any boiler might be made safe.

Do you attribute, in any manner, the explosion of that boiler to that particular alteration?—Not at all; the end, as altered, appears to me to have stood more than the end previous to the alteration.

What pressure was the boiler originally calculated to sustain?—Forty pounds to the inch.

Which

Which would you, as an engineer, recommend to be used on board steam-boats, wrought-iron or cast-iron boilers?—I think both might be used with equal safety; but that in proving them, they ought to be kept under the pressure a considerable time, say a quarter of an hour or half an hour; sudden pressure may cause flaws in a boiler, which may give rise to accident afterwards; but if under pressure a considerable time, you might see the action of it.

Mr. JOHN RICHTER'S *Evidence.*

Where is your residence?—In Cornwall Place.

What are you?—A sugar refiner.

Were you acquainted with the circumstances attending the explosion of the engine at the sugar-house in Wellclose-square?—I was.

Be so good as to state them?—I had attended from time to time during the whole period of the construction of that boiler, for the purpose of boiling sugar by means of high pressure; it was necessary we should have a pressure of from six-and-thirty to five-and-forty pounds to an inch. I saw the boiler when the bottom only was put up, and I was at that time informed that they had cast the dome part of it, and that it was not sufficient, and that they were casting another. Some months afterwards I attended, and I found that other placed there. I saw them at work, and as I went in, Mr. Haigh, who was the engineer, told me they were boiling at eighteen pounds an inch; to which I replied, that must be impossible; we have never been able to boil at less than six-and-thirty. Upon which I went to the gauge, and I found the index of the gauge standing at five- or six-and-thirty.

What was the nature of that gauge?—A mercurial gauge, intended as an index. I said, "Surely you are mistaken, this is six-and-thirty." "Oh no," he said, "that means eighteen." In consequence of which, I took an opportunity of measuring the gauge, and found the gauge to represent inches, by which I knew they were in an error. I measured to convince them of the error, but failed, and could not convince them of it till the day after the accident. In consequence of complaints from Constant, the Frenchman, in whose house it was, that it would not do its work, and his fears in pressing it on to do its work, the maker of it became anxious to show that it would, and a day was appointed for this to be done. Constant, at three o'clock in the morning, began his work, and continued boiling till about eight, but boiling with a great deal of difficulty, because he was afraid of putting the engine to the pressure he required. He gave

gave it up; he said he would boil no more, and the men in attendance, who belonged to the engineer, went to fetch the engineer. He and his men came down, and persuaded Constant to have the fire lit again. He consented, after a great deal of difficulty, and went to another pan in an adjoining building, and there he was at work when the accident happened. They were urging the steam, and actually had put an immense weight upon the lever of the valve, so as to render it totally useless. This was ascertained by a Frenchman, who saw it, and who stated to the man that he was doing mischief and doing wrong. He was told to hold his tongue and mind his own business; that he knew his business, and they knew theirs; the consequence was, that immediately afterwards it blew up. After this accident, I went every day to the ruins, for the purpose of satisfying myself of what had been the cause of the bursting; and I saw the excavation until the parts of the boiler, which was of cast iron, were found; and then finding parts of this boiler in different places, the seat of the boiler being where it had been placed, but the rest scattered about in different directions, I measured the thickness of different parts of it. The bottom of it was two inches and a half thick, the upright sides of the bottom one inch and a half thick; the lower part of the dome was seven-sixteenths thick, and one of the parts at which it must have burst, and where the boiler was completely defective in the casting, was less than the eighth of an inch thick; it was not thicker than a crown-piece: the wonder is that it stood at all, not that it burst. I am sure I never would have gone near it, if they had not assured me it was three inches thick in every part of it, and I was over it repeatedly. I apprehend the cause of that bad work was this; that the man was his own founder, as well as an engineer, and having made the thing in his own house, it was his interest to patch it up in the best way he could, and I understand it was actually patched.

Were you enabled to form any judgement to what pressure the men had raised their steam?—I could not form any judgement of that, but I understand that it had been seen at forty-eight.

What pressure was the boiler originally intended to sustain? It was not intended to be worked above forty-five, and was ordered to be made to sustain the pressure of a hundred pounds to an inch; the whole house was blown to pieces, which, I apprehend, arose from the fragments of the boiler striking the story posts, by which the support being taken away, the walls fell inwards.

Do you know whether there was a second safety-valve to this boiler?—I do not think there was.

Mr.

Mr. John Steel's *Evidence.*

Where do you reside?—At Dartford.

What is your profession?—An engineer.

Are you acquainted with the construction of steam boilers?—Perfectly so.

Will you give your opinion as to the comparative merits of wrought and cast iron?—I cannot conceive as to the safety of the two, that there is any difference whatever, when the steam is used, as it generally is for high pressure engines, to forty pounds to the inch. If it was required to make the strongest boiler imaginable, I should consider cast iron preferable, because there you can get to an unlimited strength of resistance; wrought iron you can only have of a certain thickness.

Are you of opinion, that a boiler can be made of cast metal, free from all imperfections in the substance of the metal itself?—No; I do not imagine that it can exactly, but at the same time it can be ascertained whether it is so or not before it is used.

Do you mean to say by that, that you can by any pressure say that it is free from imperfections; or do you mean to state, that it will only sustain the pressure that it is calculated for?—When boilers are proved, they are generally proved to four or five or six or eight times the pressure intended to be put on them.

But still, though they bear that pressure, they might have those imperfections?—Certainly; but without those imperfections, they would sustain, perhaps, fifty times what is wanted.

Are you then of opinion, that the proof arising from the pressure of cold water, is sufficient to ascertain the safety of a boiler which shall afterwards be exposed to the operation of fire or of highly heated steam?—Perfectly so; because I imagine it is a great deal stronger when heated to the extent steam will heat it; cast or wrought iron is at its greatest strength when it is at 300 degrees of heat, which I believe has never been arrived at yet by steam.

Supposing the interior of the cast iron to contain cavities, by which the thickness of the extenal coat is very much diminished in those parts, and that those parts shall be afterwards exposed to the action of the fire, do you apprehend then, that the apparent thickness of the boiler would be any sufficient security?—No; by no means.

Have the boilers which you have been accustomed to use been furnished with two safety-valves or one only?—Two, universally.

Has either of those been locked up from the engineer?—They sometimes have and sometimes they have not; I should imagine two-

two-thirds of them have been locked up, but I cannot exactly say; one is always exposed.

Do you think any great security is produced by the operation of a mercurial gauge, as a safety-valve?—Certainly so.

Are you of opinion, that by the adoption of those precautions, high pressure steam may be used with safety, either with wrought-iron or cast-iron boilers?—Perfectly so.

In case by accident of the explosion of a boiler—which would be attended with the greatest mischief, a cast- or wrought-iron boiler?—I should imagine the explosion would be one and the same.

Would not the cast-iron boiler be more liable to burst in fragments, than the wrought iron?—I have never seen it; I have seen several cast-iron boilers rend, but never explode.

Would not wrought-iron boilers rend?—Wrought-iron boilers rend also. It appears to me it is not from the pressure, but from the heat where the water is kept from the place where the rend takes place; I never saw a cast-iron boiler that had exploded.

Supposing two vessels, one of cast iron and one of wrought iron, of equal dimensions, which have no escape-valves at all, to be burst by the expansive force of steam; from which of those two should you expect the greatest mischief to arise?—From the cast iron.

Mr. William Brunton's *Evidence.*

What are you, and where do you reside?—I am a civil engineer, resident at Birmingham.

You are a manufacturer of steam engines?—Yes.

Have you ever manufactured any steam engines for boats?—Yes.

Have you any thing to communicate to the Committee, for their information, respecting the best construction of the engine or boiler, to produce safety to passengers on board?—Yes; I have, during the course of my experience, made several high pressure boilers, and in turning my attention to that, I was induced to examine what had been done before me; and I think we have accomplished the object, in making a boiler, which I apprehend will become useless before it becomes dangerous.

Are you acquainted with any instances of the explosion of steam boilers?—Yes, of both kinds; I know of one which exploded at Hunslet, near Leeds, whilst I was within half a mile of it; it was a low pressure boiler; the cause was the weakness of the boiler, and the effect was, that all the windows of the neighbouring manufactory, which were of lead, were torn out, and there were a great number of the work-people scalded.

Was

Was the explosion of that boiler owing to the weakness of the metal, or improper construction?—It was, perhaps, from the weakness of the metal; I cannot answer that question exactly. It was a cast-iron top; it was the upper part of the wrought iron, joining to the cast iron, that gave way. Another instance was at Sheriff-hill colliery, where the boiler was projected over the engine-house; there was no other damage done, excepting breaking a capstan.

What sort of boiler was that?—It was a round wrought-iron boiler. Another instance was at the foundry near Stourbridge, where the boiler bursted, and one man was killed.

In all the accidents you know of, did they arise from the improper construction of the boiler, or from the ignorance or mismanagement of the engine-man?—I have no doubt that either the one or the other caused all the accidents that ever happened.

Are not common or low pressure engines often used at a higher degree of pressure than was designed by the person who constructed the boiler?—Yes, and particularly in steam-boats. I have had more than once occasion to correct that, or to remonstrate with the engine-man. I should say, that this danger is considerably increased, from a number of the boilers on board the steam packets having large flat sides.

Do not the engine-men, in many cases, increase the pressure of the steam in the boiler, although it be of no additional advantage whatever in increasing the power of the engine?—Yes, I think I may say so, if applied to the low pressure engine or condensing engine. The additional force of the steam subjects the engine to a number of inconveniences.

Have you been concerned in making boilers for high pressure engines?—Yes.

Do you think that boilers for high pressure engines can be so constructed as to become useless before they are dangerous?—Yes.

Upon what principle?—Upon the principle of having the exterior part of the boiler independent of the flue, so much so, that while the flue is injured by the current action of the fire, the exterior part of the boiler remains, as to strength, unimpaired; and I conceive that a boiler thus formed, when the flue has been worn very thin, and then exposed to a greater pressure than it could sustain, the thin parts of the flue would act as so many safety-valves. From my experience in regard to these boilers, I know that when they have been worn for some time, you cannot have them tight.

You are speaking here of boilers constructed of wrought iron?—Yes; I speak of them because I have so constructed them; but I have no doubt that cast-iron boilers, if constructed upon the

the same plan, may be made equally strong, having the outside of cast iron and the inner part of wrought iron, would do the same thing.

Do you, from your own experience, believe it possible to construct boilers which will bear an expansive force of 600 pounds to an inch?—Yes; according to my experience, I have taken a good deal of pains to ascertain the strength of wrought-iron plate, and according to that I have made wrought-iron boilers that would bear 600 pounds upon an inch.

What degree of pressure have such boilers generally been worked with?—Such boilers have been worked from forty to fifty pounds upon an inch, and previously to being worked at all they have been tried with 150 pounds to an inch, by water pressure.

Are you then of opinion that there is no difficulty in constructing the high pressure boiler of wrought iron, in such a manner as to make it perfectly safe?—Yes, I am of that opinion, that the boiler may be constructed of wrought iron, with perfect safety, at a pressure of fifty pounds.

After the boiler is properly constructed, do you apply any further safeguards to it?—We adopt two safety-valves, one in an iron box under lock and key, and that is only at the control of the proprietor, and the other is open to the engine-man; and we also employ a mercurial gauge as an inverted siphon, which in the event of the steam being stronger than the mercury can sustain, the mercury will be driven out, and the boiler thereby relieve itself.

Do you consider this mercurial gauge in any other light than as an additional safety-valve, or as a contrivance by which notice is given of the pressure growing too high?—In both these respects I employ it; I consider that in both those two points of view it is useful.

Are you of opinion, that if the common safety-valves be properly adapted, the mercurial gauge may be dispensed with; when I say properly adapted, I mean sufficient in number and capacity, and one of them completely secured from the intermeddling of the engine-man?—I should think it would be safe.

What do you think respecting the comparative mischief probably to arise from the bursting of a high pressure or a low pressure boiler?—In the high pressure boiler the injury would be done principally by the fragments projected; in the low pressure boiler, the mischief may arise chiefly from the hot water and steam. I may mention two instances in illustration of this; the first, of a low pressure boiler having given way in the bottom, when a stream of hot water was projected against the engine-man, causing his death; the second instance was of a high pressure boiler, in which a hole was suddenly opened, the water projected

jected itself and completely wetted a boy standing within a yard of the orifice, who was not at all injured thereby. I should say, the fragments from the cast-iron boiler would be, for any thing that I know, equally destructive either with a high or with a low pressure.

What injury do you think is likely to arise from the bursting of a high-pressure boiler composed of wrought iron?—I conceive the injury would be more partial, in consequence of the fragments being larger; for I do not suppose that the wrought-iron boiler would be divided into so many parts as a cast-iron boiler would.

Do you apprehend, that a wrought-iron boiler would burst in the same manner with a cast-iron boiler; I mean, whether the manner of bursting would be the same?—Yes, I think it would.

Supposing that cast-iron boiler to be burst by the expansive force of the steam, does it usually rend, or go into fragments?—Cast iron will go into fragments.

What would be the effect of the same force which would produce explosion upon a wrought-iron boiler?—The probability is, that there would be much fewer fragments of the wrought-iron boiler;—perhaps only two.

Does not the greater tenacity of the wrought iron prevent the fragments from being carried off in the same manner as when the cast-iron boiler bursts?—No; I presume, that if the wrought-iron boiler bursts, whatever fragments there are, they are completely detached from that boiler, and they will go as far and do as much mischief as those of a cast-iron one.

Are the fragments separated from the wrought-iron boiler by explosion, in the same manner as they are from a cast-iron boiler?—Yes; they would be projected with equal force, under equal circumstances. When I say that the wrought iron will rend, I am also of opinion, that a part of it may be projected: I have an immediate eye to the circumstance of one part of it being separated, and that the one part would be carried with as much violence in the cast iron as in the wrought iron.

Is there not a greater probability in the wrought-iron boiler rending, and not separating into fragments?—I know that one wrought-iron boiler burst with a high pressure steam; and a fragment, the largest piece, was carried to the distance of 150 yards.

Was that a piece of the wrought iron?—Yes.

Have you any thing to add to that part of your answer?—No.

You have said that the boilers which you manufacture, are generally made of wrought iron; what is your reason for preferring the wrought to the cast iron?—I was induced from the examination of several cast-iron boilers, which I found cracked or

or broken in the lower part of them, which in my opinion arose from the unequal temperature and expansion in the exterior part of the boiler; this unequal temperature is caused by a quantity of water at all times under the flue, and consequently of lower temperature than the water above the flue; thereby causing the upper part of the boiler to expand in a greater ratio than the under part of the boiler, which in my opinion caused the fractures alluded to. This circumstance induced me to make use of wrought-iron boilers as I have explained or described, in preference to the others.

In a steam boat, what boiler would you most recommend to be used to insure safety to the persons on board; a wrought-iron or a cast-iron boiler?—A wrought-iron boiler, properly constructed.

What safety-valves would you recommend to be placed to boilers on board steam-boats, to insure the greatest safety, or to guard against the boiler's exploding; I mean as to number?—I recommend at least two safety-valves; the one to be placed under the lock and key of the proprietor of the vessel, so secured as not to be accessible to the engine-man; and one which the engine-man has the usual control of.

Have you any thing to recommend with regard to the particular construction of these safety-valves, so as to insure their acting in constant operation?—I would recommend the valve to be nearly flat, or quite so, which I apprehend would be less liable to be fastened by the difference of temperature to which the valve and the seat might occasionally be subjected.

I suppose such a safety-valve would not be liable to be impeded by much friction?—As little friction as perhaps can be.

You have not any thing particularly to suggest?—No.

[To be continued.]

XLIII. A short Account of Horizontal Water-Wheels. By ADAMSON[*], Esq.

On perusing the works of mechanical writers, it appears, that many attempts have been made to construct horizontal water-wheels, on such a principle as would give them sufficient power for mechanical purposes; but that these attempts have often failed.

The principal kinds, of which we have any account, are:

1. Such as have their vanes or floats placed round the rim, like those of a wind-mill, and which are made much broader than the vein of water which is to strike them; the water is delivered

[*] Communicated by the Author.

livered from a spout, which is so directed as that they may be struck in a direction perpendicular to their surface.

2. Those which have their floats ranged round the rim of the wheel in planes inclined to the radius, but parallel to the axis.

3. Those which have the floats standing on a sole, or on the side of the rim, not pointing to the axis, but aside from it, so that they will admit of the spout being more conveniently placed.

4. The centrifugal wheel, commonly called Barker's mill.

This consists of an upright pipe or trunk, communicating with two horizontal arms, each having a hole near the end opening in opposite directions, and at right angles to the arms. The water is poured from a spout into the top of the trunk, and issues through the holes in the arms, with a velocity corresponding to their depth below the surface of the water; by which the arms are forced backwards, and a retrograde motion is given to the wheel.

5. In the year 1797, a patent was taken out by Mr. Robert Beatson, for a method of constructing horizontal mills to go either by wind or water. The machine consists of four rectangular frames or wings, standing at right angles to each other on an upright shaft. The floats, which consist of any thin light substance, are fixed in the frames perpendicular to the horizon, and are so constructed, that when they face the wind or the current of water, they are shut, and fill up the whole space within the frame; but on the opposite side, when they get against the current, they are open, and permit the wind or water to pass between them.

This machine, as a water-mill, was intended to act in the current of a river, or by the ebbing and flowing of the tides.

These seem to be the principal kinds of horizontal wheels; and from the nature of the principles upon which they act, it is evident their powers must be very small.

It however appears that many are in use on the continent of Europe.

An Explanation of the New Patent Horizontal Water-Wheel, and the Principles of its Action.

A circular wall, in the form of a hollow cylinder, is built in a perpendicular position on a horizontal plane.

Through the side of the cylinder, at the bottom, several rectangular cuts or passages are made, the sides of which are perpendicular to the base, or bottom of the cylinder, and the length of each within, is about four times the width. Fig. 1, Plate IV.

The passages or cuts are made quite round the circumference, and so near to each other, that the sections of their sides within, make an acute angle, and leave, between each two, a solid part

in the form of a wedge, the edge of which is perpendicular to the base, so that a line drawn from the centre of the wheel to it, will form a right angle with that side of the cut which faces the centre. Fig. 1.

Within the cylinder is placed the horizontal wheel, with floats, and a perpendicular axis or spindle, which turns on a point in the centre. Figs. 1 and 2.

The floats FF are rectangular planes, fixed round the edge of the wheel in planes passing through the centre, and perpendicular to the plane of the wheel. Their height is something greater than that of a cut, and their breadth rather more than its width: also their number may be about three times the number of cuts. But for the purpose of obtaining the most regular motion, the numbers of the cuts and floats ought to be prime to each other. Fig. 2.

The cylinder is surrounded by a reservoir of water, supported by a circular wall, which, in low falls, may be equal to its depth. Fig. 1.

The reservoir is filled, from the canal or river, by a stream flowing through a head or slit at the top of the outer wall, and at the bottom. The water flows through the cuts PP against the floats, and turns the wheel. Figs. 1 and 2.

The width of the cylinder within, RR, is continued downwards below the floats FF, to a depth sufficient for permitting passages to be made under the reservoir, of sufficient capacity to take away the water as fast as it enters the inner cylinder. Fig. 2.

The passages at the bottom of the machine, showing the escape of the water, appear in the plate, for the want of room, to occupy only half the circumference, but ought to be continued quite round. Fig. 2.

In fig. 2, where part of a perpendicular section of the machine is represented, the passage of the water appears to be only on one side, but the opposite side is supposed to pass through one of the solids which supports the reservoir and wall.

The wheel, to about half the radius, is open quite round the centre, for the purpose of permitting the free passage of the air; (this, in a large wheel, may be much more than half;) the remainder is solid, quite round, and curved or dished on the under side, for the purpose of turning the water downwards, and preventing it from rising above the wheel, as it passes from the float, in a thin sheet to the centre, where it forms a head, which by its pressure facilitates its escape. Fig. 2.

According to the manner in which the floats are fixed in the wheel, they ought, in the figure, to be invisible; but are made to appear, for the purpose of showing the nature of the action of the water against them. Fig. 1.

To

To find what depth the bottom passages ought to be, it will be only necessary to know the breadth and depth of the head through which the water flows into the reservoir, as the same quantity must pass both places in the same time.

The perfection of this machine may be shown as follows:

1. The floats being open on all sides, except that opposite the centre, will prevent as much as possible, any reaction against the water coming in.

2. The space below the floats, and the passages from it, being always sufficient to take away the water as fast as it enters, will prevent any accumulation of tail water from impeding the floats.

3. The velocity of the water being greater than that of the wheel, prevents any impediment by centrifugal force.

4. The force of the water through the cuts, arises from its perpendicular pressure from the surface to the centre of force, and therefore is the greatest possible.

5. The line of pressure against the floats, is as nearly perpendicular to their surface, and as near to the extremity of the radius, as it is possible to make it act against the floats of a wheel, and therefore the pressure against them cannot be greater.

6. The water acts against all the floats at the same time.

7. The whole of the water acts against the floats.

8. The water receives no check from the want of air.

9. No water-wheel can move with less friction.

Hence it must be evident, that these principles will give the greatest power that can possibly be obtained from the action of water upon a horizontal wheel:

But as a wheel acting on these principles has never before been tried, it was thought most advisable to put it to the test by experiment, previous to making it public. A very complete and perfect model (or rather a little mill) has therefore been made by Messrs. Bramah and Sons, at their manufactory in Pimlico, near London.

The Model

stands on a base of two feet diameter, and its height is 53 inches.

The outward cylinder, which supports the water in the reservoir, is of cast iron.

The inner cylinder, in which the wheel moves, is of wrought iron, and its lower end, through which the cuts of water passages are made, is of brass.

The depth of the reservoir is about 51 inches.

The number of cuts or water passages is 24, and their depth one inch.

The wheel and floats are of brass.

The diameter of the wheel is 12 inches, and the number of floats is 79, a prime number.

 A ma-

A mahogany wheel or pulley of equal diameter to the wheel is fixed on the top of the spindle, and above it one of about 6·8 inches diameter is fixed, for the purpose of making experiments.

The water escapes at the bottom quite round the machine.

Experiments.

With this model or mill, the following experiments were made.

When the reservoir was full to above four feet above the centre of pressure, or middle point in the cuts, the wheel made nearly four revolutions in a second, and, as no weight was then suspended, this was its greatest velocity.

A cord was then fixed to the smaller wheel, and passed over a pulley, with a weight suspended, when twelve revolutions of the wheel made in

$$\left.\begin{array}{r}25\\13\\6\\5\end{array}\right\}\begin{array}{l}\text{seconds}\\\text{raised}\end{array}\left\{\begin{array}{r}12\\10\\8\\6\end{array}\right\}\text{pounds, 21 feet, or}\left\{\begin{array}{r}50\cdot4\\96\cdot92\\210\cdot\\252\cdot\end{array}\right\}\begin{array}{l}\text{feet in a}\\\text{minute.}\end{array}$$

Then each weight multiplied by the height to which it was raised in a minute gives the momentum; therefore

$$\left.\begin{array}{l}12\times50\cdot4=604\cdot8\\10\times96\cdot92=969\cdot2\\8\times210\cdot=1680\cdot\\6\times252\cdot=1512\cdot\end{array}\right\}=\text{the momentum.}$$

Hence it appears, that the third experiment produced the greatest effect, and that the wheel then made twelve revolutions in six seconds, or two in one second, and therefore it moved with nearly half of its greatest velocity. Consequently, when the wheel moves with nearly half of its greatest velocity, it works to the greatest advantage, supposing the third experiment to be the maximum.

Diameter or Size of the Wheel.

This wheel may be made of any diameter that may be required for making a given number of revolutions in a given time.

Velocity.

The wheel may move with any velocity whatever that can be obtained from the fall.

Mr. Banks, at page 105 of his Treatise on Mills, by taking a mean of the experiments made by six different authors, for the purpose of finding with what velocity water will issue from a fall of a given depth, gives 5·4 × square root of the depth = velocity of the water.

But according to these experiments, 6 comes much nearer than 5·4, and also agrees exactly with the experiments made by Banks himself; and as, in these experiments, it gives nearly the velocity

velocity of the wheel, therefore 6 × square root of the depth = velocity of the wheel, and this may also, in practice, be taken for the velocity of the water without any material error, though its velocity will always be something greater than that of the wheel when moving without resistance.

On these principles a small wheel with a high fall will move with a velocity amazingly great. Thus, let the diameter of the wheel be one foot, and the height of the fall eighty-nine feet, then $6\sqrt{89} = 56 \cdot 60388$ feet, the velocity per second; and as the circumference of the wheel is 3·1416 feet; therefore

As 3·1416 : 1 : : 56·60388 : 18 revolutions per second.

or 18 × 60 = 1080 revolutions in a minute.

Power.

In the specification, the power of the horizontal wheel was compared to that of the overshot, on a supposition that the force of a stream of water acting against a perpendicular plane near the orifice from which it flows, is nearly equal to the weight of the column which impels it, as Mr. Banks has proved by experiment.

But in making some experiments for the purpose of ascertaining the manner in which the water acts against the floats of the horizontal wheel, it appeared,

That if a stream of water from a horizontal pipe, act against a perpendicular plane near the orifice with any considerable force, it will spread quite round in a thin sheet parallel to the plane, and leave it on all sides in that direction; and

That if the edge of the stream be brought a little beyond the edge of the plane, so that part of it may pass by, it will form an angle with it; and that as the further side of the stream approaches the edge of the plane, the angle will increase until they coincide, when it will become a right angle.

Hence it is evident, that there is a reaction in this machine against the water coming in, which it is impossible to avoid, and that this is what reduces its power below that of the overshot wheel; but that this reaction is very different from the centrifugal force.

Before we proceed to compute the power of the wheel, it is necessary to observe, that when the radius is one, the width of a cut is equal to the natural versed sine of the angle between two of them, taken at the centre, and therefore,

If the versed sine of the angle between two cuts be multiplied by any given radius, the product will be the width of a cut to that radius; and since all the cuts, in any cylinder, are equal in width, as they are also in depth; therefore,

If the versed sine of the angle between two cuts be multiplied

 by

by the radius, and then by the number and depth of the cuts, that is versed sine × radius × number × depth, it gives the area of a rectangular section equal to the area of the perpendicular sections of all the cuts.

In the model the radius is six inches, the number of cuts twenty-four, and their depth one inch; the angle 15°, and its versed sine ·034074; therefore

$$\cdot 034074 \times 6 \times 24 \times 1 = 4 \cdot 906656 \text{ square inches,}$$

which, in consequence of the cuts having been made rather wider by dressing, is taken at five square inches or $\frac{5}{144}$ square feet, and the water being four feet deep, its velocity was $6\sqrt{4} = 12$ feet per second;

Hence, $\frac{5 \times 12}{144} = \frac{5}{12}$ cubic feet of water issue in a second, or $\frac{5 \times 60}{12} = 25$ cubic feet in a minute.

Therefore for the power, we have 25 cubic feet, or $25 \times 62 \cdot 5$ pounds of water descending through four feet in a minute; hence

The momentum of the power is $25 \times 62 \cdot 5 \times 4 = 6250$.

Then to find the momentum of the effect, according to Mr. Smeaton's method;—when the wheel moved without water, a weight of ten ounces gave it a velocity of two revolutions per second. Therefore according to the third experiment, the weight raised was eight pounds ten ounces, or 8·625 pounds; consequently,

The momentum of the effect was $8 \cdot 625 \times 210 = 1811 \cdot 25$ and as 6250 : 1811·25 : : 1 : ·2898 the effect. But if the velocity of the water be found according to Mr. Banks's mean of the experiments of six different authors, it will be 10·8 feet per second, and the effect will be ·322; and this makes the power of the horizontal wheel double to that of the undershot, according to the second example in Mr. Smeaton's Table.

Remark.

Mr. Smeaton, at page 12 of his Treatise on Mills, gives an account of an experiment on the undershot wheel, where it appears that his head, or fall, of water was thirty inches, and that 264·7 pounds weight of water was expended, or descended through thirty inches in a minute; hence,

The momentum of the power was $264 \cdot 7 \times 30 = 7941$, that 9·375 pounds weight of water was raised through 135 inches in a minute by the wheel; hence,

The momentum of the effect was $9 \cdot 375 \times 135 = 1265 \cdot 625$, therefore as 7941 : 1265·625 :: 1 : ·1594 the effect, and $\cdot 1594 \times 2 = \cdot 3188 =$ double the effect.

But

But it appears that Mr. Smeaton has inserted ·32 in his Table as the true effect in this case, on a supposition that the same effect may be obtained from half the power; and he therefore multiplies the weight of the water expended in a minute by 15, or half the depth, instead of 30, which was the depth through which the water, that turned the wheel, actually descended in a minute.

Had he made such a discovery as this, he ought to have given a demonstration, or a clear proof of its truth; for his argument about a *virtual head*, certainly gives no such proof: on the contrary, he says that he has obtained more than double of what is assigned by theory; and that this is very different from the opinions and calculations of authors of the first reputation.

The reason of making this remark is, that it is probable the power of the horizontal wheel will be compared with that of the undershot, according to Mr. Smeaton's Table, where he has inserted double the power of the undershot wheel (or very near it) according to his own experiments.

The horizontal wheel may be used in any fall however high or low.

In low Falls.

Example.—Let the depth of the fall be two feet, diameter of the wheel twenty feet, number of cuts twenty-four, and their depth four inches;

Then, by the Table, the angle between two cuts is 15°, and its versed sine ·034074; therefore,

$\cdot 034074 \times 10 \times 24 \times \frac{1}{3} = 2\cdot 72592$ square feet, or the area of a rectangular passage equal to that of the perpendicular sections of all the cuts.

This may therefore be considered as the base of a column of water, the height of which is the perpendicular distance from the surface to the centre of pressure or the middle point of the cut, which in this case is 22 inches, or $\frac{11}{6}$ feet; hence we have

$2\cdot 7592 \times \frac{11}{6} = 5$ cubic feet, nearly $= 5 \times 62\cdot 5 = 312\cdot 5$ pounds weight constantly impelling the water through the cuts against the floats quite round the wheel, and $312\cdot 5$ divided by 24, gives 13 pounds for each cut or passage. The greatest velocity of the wheel will be $6\sqrt{\frac{11}{6}} = \sqrt{66} = 8\cdot 124$, or about eight feet per second; and therefore when it works to the greatest advantage will be four feet per second. Then

as $4 : 1' :: 20 \times 3\cdot 1416 : 15''\cdot 7$ time of a revolution.

In high Falls.

In order to obtain the full force of the water here in the same

manner as in low falls, the height of the walls of the reservoir would require to be equal to that of the fall. But,

This however is not necessary, as both the reservoir and inner cylinder may be covered at any proper height, as denoted by the dotted line in the plate, but the reservoir must be made water-tight.

A pipe may then be brought from the surface of the water to the bottom of the reservoir, where it must be so fixed that the water may flow from it in the same direction as the wheel turns, which, in that respect, will augment the power.

But as this supplying pipe will be in the place of a reservoir of water, the area of a section of it ought to be greater than the sum of the areas of the perpendicular sections of all the cuts, and it ought also to be constantly full up to the top, otherwise the water would not be supplied so fast as it could pass through the cuts, and a part of the power would be lost, unless there were a contrivance for covering or shutting up part of the cuts.

Example.—Let the depth of the fall be 81 feet, diameter of the wheel 10 feet, number of cuts 30, and their depth half a foot.

Then, by the Table, the angle between two cuts will be 12°, and its versed sine ·021852; therefore,

$\cdot 021852 \times 5 \times 30 \times \frac{1}{2} = 1 \cdot 6389$ square feet, which is the area of a rectangular passage, equal to that of the perpendicular sections of all the cuts, and the diameter of a circular pipe of equal area will be 17·3 inches, therefore the diameter of the supplying pipe must be greater than this.

If the radius of the wheel and depth of the cuts remain the same, the greater the number is, the less will the area of the whole of their perpendicular sections be, and consequently, the less water will pass through them, but it will act nearer to the circumference; and therefore, in proportion to its quantity, will produce a greater effect.

Example.—Let the numbers be 12, 16, 30, 50, then these multiplied by their respective versed sines will be

$$\left.\begin{array}{l} 12 \times \cdot 133975 = 1 \cdot 6077 \\ 16 \times \cdot 076120 = 1 \cdot 21792 \\ 30 \times \cdot 021852 = 0 \cdot 65556 \\ 50 \times \cdot 007885 = 0 \cdot 39425 \end{array}\right\} \text{which are the ratios of the sums of the areas of their perpendicular sections.}$$

Hence, when the quantity, or supply of water is great, the number of cuts must be small, and, on the contrary, when it is small, the number of cuts must be great in order to obtain the greatest effect.

The

The following Problems may sometimes be useful:

PROB. 1. Given the angle between two cuts, to find the number of cuts.

Rule. Find the angle in the table, and against it stands the number.

PROB. 2. Given the number of cuts, to find the angle between two.

Rule. Find the number in the table, and against it stands the angle.

PROB. 3. Given the angle between two cuts and the radius of the wheel, to find the width of a cut.

Rule. Multiply the versed sine of the angle by the radius, and the product is the width of a cut.

PROB. 4. Given the number of cuts and the radius of the wheel, to find the width of a cut.

Rule. Find the versed sine (against the number) in the table, and multiply it by the radius for the width.

PROB. 5. Given the angle between two cuts and the width of one, to find the radius of the wheel.

Rule. Divide the width of the cut by the versed sine of the angle, and the quotient is the radius.

PROB. 6. Given the number of cuts and the width of one, to find the radius of the wheel.

Rule. Find the versed sine (against the number) in the table, by which divide the width, and the quotient is the radius.

PROB. 7. Given (D) the depth of the fall, and (d) the diameter of the wheel, both in feet, or both in inches, to find the number of revolutions in a given time.

Rule. Take $\frac{6\sqrt{D}}{3{\cdot}1416d} = \frac{\sqrt{D}}{{\cdot}5236d} = n =$ number of revolutions in a second; then $n \times$ number of seconds in the given time gives the number of revolutions in that time.

Example. Let $D=45$ feet and $d=5$ feet; then $\sqrt{45} \div {\cdot}5236 \times 5 = 2{\cdot}562$ revolutions in a second $= n$, and $2{\cdot}562 \times 60 = 153{\cdot}72$ revolutions in a minute.

PROB. 8. Given (D) the depth of the fall, and (n) the number of revolutions in a given time, to find the diameter of the wheel.

Rule. Find (n) the number of revolutions in a second:

Then since $\frac{\sqrt{D}}{{\cdot}5236d} = n, \therefore \frac{\sqrt{D}}{{\cdot}5236n} = d.$

Example. Let $D=30$ feet, and the number of revolutions in

in a minute $=138$, then $138 \div 60 = 2{\cdot}3 = n$, and $\sqrt{30} \div {\cdot}5236 \times 2{\cdot}3 = 4{\cdot}55$ feet, the diameter required.

It may be proper here to observe, that when the quantity of water is not too great, nor the fall too high nor too low for the overshot wheel; its power will exceed that of the horizontal; yet, in general practice the horizontal will certainly be superior, for the following reasons:

1. Because the horizontal will act in any fall, its friction will not increase by the increase of water, and as it receives the water quite round the circumference, it will (when the supply is sufficient) work with a quantity greater than can be applied to the overshot without great loss of power.

2. In the horizontal, while the depth from the surface of the water to the centre of force in the cuts remains the same, the power will increase with the quantity of water acting against the floats, or as the depth of the cuts; and since the quantity of water increases also with the circumference, or the radius of the wheel: Therefore

The power will be as the product of the radius and depth of the cuts.

Thus, if the depth of the cuts be made three times as great, and the radius twice as great, the power will be $3 \times 2 = 6$ times as great. Hence

If, in the model, the depth of the cuts be made 10 inches, and the radius 60 inches, or ten times as great, the power will be $10 \times 10 = 100$ times as great, though the depth of the fall would be increased only $4\frac{1}{2}$ inches.

3. When the depth of the fall is given, the size of the overshot, as also its velocity, is fixed; for if its diameter be 16 feet, its velocity, to produce the greatest effect must be five feet per second; but the velocity of the horizontal wheel, with a fall of 16 feet, must be 12 feet per second. Again, an overshot of 36 feet diameter must move 5·33 feet per second; but a horizontal with a fall of 36 feet must move with a velocity of 18 feet per second, to produce the greatest effect.

4. The overshot must have a wheel fixed on its axis, and connected with other wheels or machinery before any effect can be produced; but in the horizontal this is sometimes not necessary, as a mill-stone may be fixed on the top of the axis, and made to revolve with a proper velocity, without any connexion with other wheels.

TABLE

TABLE *showing the Angle between two Cuts with its natural versed Sine from 9 to 52.*

	Angle.	V. Sine.		Angle.	V. Sine.
	° ′			° ′	
9	40 0	·233956	31	11 36·79	·020470
10	36 0	·190983	32	11 15·	·019215
11	32 43·63	·158746	33	10 54·55	·018071
12	30 0	·133975	34	10 35·29	·017027
13	27 41·54	·114544	35	10 17·14	·016070
14	25 42·86	·099031	36	10 0	·015192
15	24 0	·086454	37	9 43·78	·014384
16	22 30	·076120	38	9 28·42	·013639
17	21 10·59	·067528	39	9 13·85	·012950
18	20 0	·060307	40	9 0	·012312
19	18 56·81	·054183	41	8 46·83	·011720
20	18 0	·048943	42	8 34·28	·011169
21	17 8·57	·044427	43	8 22·33	·010657
22	16 21·82	·040507	44	8 10·91	·010179
23	15 39·09	·037083	45	8 0	·009732
24	15 0	·034074	46	7 49·56	·009314
25	14 24·	·031417	47	7 39·57	·008923
26	13 50·77	·029058	48	7 30·	·008555
27	13 20·	·026955	49	7 20·82	·008210
28	12 51·43	·025072	50	7 12·	·007885
29	12 24·83	·023379	51	7 3·53	·007580
30	12 0	·021852	52	6 55·39	·007291

W. ADAMSON, Ebury-street, Five-fields, Chelsea,
August 20, 1817.

XLIV. *On Ebbing and Flowing Springs; with Geological Remarks and Queries.* *By* A CORRESPONDENT.

To Mr. Tilloch.

SIR, — IN the Number of your Magazine for August, Mr. Inglis seems to have satisfactorily accounted for the ebbing and flowing spring of fresh water at Bridlington quay, by the pressure of the sea upon a stratum of flexible clay which divides the fresh from the sea water.

His assertion that this bed of clay extends to the Spurn Point is probably correct; but he is not so, in supposing that it will be found to rise and fall with the ebbing and flowing of every tide; at least that effect is not produced in the neighbourhood of Hull, at

at which town I resided several months about twenty years ago. I was then assured that no pure fresh water could be had there, but from under a stratum of clay which at the Block-house mill was at the depth of about ninty-eight feet, and is supposed to basset or out-crop a few miles west of Hull. I made some inquiries as to the stratification in those parts, and was told that the hard chalk rock of which Flamborough Head is composed, is at the surface a little to the west of Hull, and from thence is said to dip E. to Spurn Point, and SE. into Lincolnshire, at the rate of five yards per mile. The strata incumbent upon it reckoning downward are; viz.

1st. Soil or earth, two feet.

2d. Warp, twenty-two feet, being about the height of the highest tide at Hull.

3d. Morass, about three feet, in which are found decayed vegetables and large trees.

May not this morass be connected with the submerged forest near the mouth of the Humber on the Lincolnshire coast? See Phil. Transactions for 1799, part i.

4th. Alluvial, at Hull about seventy feet, consisting of sharp loose sand, carbonated wood, chalk, &c. below which is a stratum of compact white clay more or less thick, between which and the chalk rock is lodged the only pure water to be got in that neighbourhood.

At Sproatley, the chalk rock is supposed to be 198 feet below the surface.

Swanland and Riplingham hills, to the west of Hull, are reported to be chalk with alternate layers of flint 6 to 8 inches thick. The latter hill is 400 feet above the level of the Humber, and is said to have been penetrated 50 feet below it.

As all the Yorkshire wolds are chalk hills, it is not probable that water could be there procured by boring, as suggested by Mr. Inglis; but in the neighbourhood of Hull, and to the east, it is practicable. In November 1798, I visited a farm-house about three miles from Hull, and about a quarter of a mile on the left of the road leading from thence to Beverley. Four months before, they had sunk a well and bored for water; and at the depth of 58 feet came upon a spring which had to that time *invariably* thrown up, to the height of two feet above the surface, a column of pure soft water which discharged more than twenty gallons per minute*. I have not since had an opportunity of ascertaining whether this spring continues to furnish a supply of water; but at Sheerness and Wimbleton it is well known that wells sunk to much greater depth have continued to afford

* They told me forty gallons, but I wish to be within compass.

a con-

a constant supply, though not to the surface. I was induced to communicate these particulars to you, in the hope that some of your intelligent readers resident in that vicinity, or others that have visited it, may be competent to furnish more correct or further information on this subject.

I am, sir,
Your most obedient servant, and constant reader,

Wakefield, Sept. 10, 1817. W. S.

XLV. *On forming Collections of Geological Specimens; and respecting those of Mr.* Smith *in the British Museum.*

To Mr. Tilloch.

Sir,—It cannot fail to be a source of pleasure to every one to witness the progress of discovery, particularly in those sciences which are of real use to mankind: and the more so, when the nature of the science is such, that there are few men of observation who cannot contribute their mite towards its progress.

Accordingly, we find that most of the useful sciences have at one time or another become objects of general attention, occasioned either by some fortunate discovery, or happy simplification of an apparently difficult branch of study. We are glad to find that geology, among the rest, is now beginning to share the attention of men of science, being well aware that its improvement will be the natural consequence.

One great step towards this improvement, will be the forming of collections of specimens, on such principles as are best adapted to the purpose of identifying the superficial strata of the earth; and of elucidating the nature of their formation, and of the gradual changes which have taken place on the surface of this planet.

It cannot, however, be expected that the relative position or identity of a stratum is to be determined, with certainty, from any single character; therefore it is obvious that a geological collection of specimens must differ materially from a collection of minerals. For the mineralogist, a simple specimen of each mineral substance is sufficient—but a fossil shell, petrifaction, or mineral is useless to the geologist, unless it be accompanied with a proper description of the stratum, and of the exact place from whence it was obtained: hence it is necessary that a descriptive catalogue should always accompany a collection of *geological* specimens.

Mineralogy is an art that becomes more curious than useful, unless it be connected with geology or chemistry; but its usefulness

ness to either of these sciences is unquestionable*. In geology, however, it is as likely to mislead, as to conduct us to the proper end of our researches, unless it be directed to its proper object.

The most important, and by far the most interesting part of geology, is that which describes and determines the relative ages of the strata which form the superficial crust of the earth;—to these strata we must look for a history of the changes which the surface of the earth has undergone.

The limited powers which we possess of gaining information, renders it necessary that we should examine with the most careful attention the means which we have, and that we should apply them in the best manner to account for the phænomena.

The nature of the organic remains that are found imbedded in many of the strata, appears to have been considered capable of throwing some important lights on this subject, by many writers, who seem, however, to have had no correct ideas respecting the manner of rendering this kind of knowledge useful, and their statements are general and incorrect; such as must ever arise from limited and hypothetical views of a subject.

It is to the meritorious exertions of Messrs. Smith, Sowerby, and Parkinson, that we are chiefly indebted for the true application of mineral conchology in explaining the structure of the earth; but more particularly to Messrs. Smith and Sowerby, who have directed their attention to the subject, with the view of rendering it useful in identifying the strata.

Mr. Parkinson's "Organic Remains of a Former World" has been some years before the public: in this work he has given the localities of many shells, but not often their places in the strata.

Mr. Farey has laid before your readers † an alphabetical list of the *places* where the shells were found, that are described in the first volume of Mr. Sowerby's "Mineral Conchology," with the situations of the places, the names of the shells, and the places in the British series of strata to which they belong—this latter object having been but imperfectly accomplished in the text of the "Mineral Conchology."

Since that period Mr. Sowerby's collection has been much increased by the contributions of the friends of science, and he has now published the xxxth number of his "Mineral Conchology."

* Many valuable analyses of minerals are extremely unsatisfactory, from the want of a correct description of the specimens analysed. This neglect has been very justly censured by an able chemist, (Annals of Phil. No. 52, p. 332,) whose example, in this respect, is well worthy of imitation; as well as his manner of describing minerals, which is a modification of that followed by the excellent Kirwan.

† Philosophical Magazine, vol. xlvi. p. 211.

Also,

Also, in consequence of some pecuniary assistance from Government, Mr. William Smith has begun to lay the result of his researches before the public*. This assistance has been given Mr. S. on condition that he arranged and placed his collection of fossil shells, &c. in the British Museum, for the use of the public. He has already published several numbers of his "Strata identified by organized Fossils," containing engravings of the most characteristic shells of each stratum; and also the first part of his "Stratigraphical System of Organized Fossils †," referring to the specimens in the British Museum.

The latter work describes the principal shells found by him in the British series, from the uppermost down to the lias strata: and with this communication you will receive an alphabetical list of the places where these shells were found, with the number of species from each place. The object of this list is to direct the attention of collectors to the places from whence specimens are most likely to be obtained; both to enable them to repeat the observations of preceding inquirers, and to extend their observations to other places ‡. X and Z.

An Alphabetical List of the 263 Places which supplied Mr. W. Smith with the 1155 Specimens of *Fossil Shells* above the *Lias* Strata, that are deposited in the British Museum, and described in the first part of his "Stratigraphical System."

	Species.		Species.
Abbotsbury	1	Bath-Easton	1
Aldborough	12	Bath-Hampton ..	2
Alderton	3	——— foot of plain	1
Alfred's Tower ..	1	Bayford, S. of, ..	11
Ancliff	4	Bentley	4
Bagley-Wood Pit ..	11	Black-dog Hill, near Standerwick	1
Banner's Ash	7		
Bath, near, ...	33	Black-down	9

* It is to be regretted that Mr. S. had not encouragement sufficient to induce him to publish those works sooner, as we understand he has long been in possession of the materials; indeed they form the basis of his great work "The Map of the Strata of England and Wales;" and therefore he must have been far advanced in those inquiries when he began that arduous undertaking.

† We shall be better pleased with this work when it is furnished with a copious index to both the shells and places, with the bearing and distance of each place from some principal town or village. Also a simple outline engraving of each shell would be very desirable. These additions would add much to the real value of the work, without materially increasing the expense,—an expense which might perhaps have been lessened in some other respects.

‡ See Phil. Mag. vol. xlv. p. 277-279, where some judicious remarks are made on collecting fossil shells, &c. &c.

Bognor

	Species.
Bognor	5
Bracklesham Bay	2
Bradford	7
——— Lock	2
——— in Canal	1
Bramerton	17
Bratton-Turnpike	3
Brightwell	6
Brinkworth-Common	5
Brixton-Causeway, Well	1
Broadfield Farm	9
Bruham	2
Bruham-Pit (Coal Trial)	1
Bubdown	[illegible]
Burgh-Castle	[illegible]
Burnham-Overy	1
Bury	1
Bury St. Edmund's	1
Caisson	1
Calne	4
Carshalton	1
Castle-Combe	5
Charlton-Harethorn	8
Charmouth	1
Cherry-Hinton	1
Chesterford	1
Chicksgrove	3
Chipping-Norton	6
Chiltern	4
Christian-Malford	2
Churchill	21
Chute-Farm	37
Clayton-Hill	1
Closworth	8
Coal-Canal	22
Combe-Down	2
Combhay	6
Cotswold-Hills	4
Crewkerne	1
Cricksley-Hill	3
Crockerton	1
Cross-hands	1
Croydon	1
Damerton	1
Danby-Beacon, near,	1

	Species.
Dauntsey-House (in stone)	4
Derry-Hill	8
Devizes	2
Devonshire Buildings, Bath	1
Didmarton	1
Dilton	1
Dinton-Park	1
Dowdswell-Hill	1
Down-Ampney	1
Draycot	4
Drysandford	1
Dudgrove-Farm	6
Dundry	12
Dun's Well, *see* Stilton-Farm	
Dunkerton	1
Dursley	1
Elencross	1
Ensham-Bridge	1
Enstone	5
Even-Swindon	1
Evershot	2
Farley	29
Fonthill	3
Foss-Cross	1
Foxhole	9
Frocestor-Hill, top	2
———— foot	1
Frome, W.	1
Fullbrook	4
Gagen-Well, near	2
Glastonbury	2
Gloucestershire	1
Godstone, near,	3
Grassington-Hill	1
Great-Ridge	1
Grimston, near,	3
Gripwood	7
Guildford	8
Happisburgh-Cliff	4
Hardington?	4
Heddington Common	4
Heytsbury	9
Hickling	1
Highgate Archway	1
Highworth	1

Hill-

	Species.
Hill-Marton	5
Hinton	9
Hinton-Waldrish ..	3
Hogwood-Corner ..	1
Holkham-Park ..	1
Holt	4
Hordel-Cliff	1
Hurstanton-Cliff ..	2
Ilmington	1
Kelloways	11
Kennet and Avon Canal	3
Kennington	6
Kent (County) ..	1
Keswick	1
Knook	1
——— Castle and Barrow	1
Lady-Down	1
———— Farm ..	1
————, on Biss river	1
Landsdown	2
————, near, ..	1
Latton	2
Laverton	2
Leighton-Buzzard ..	1
Leiston	1
——— Old Abbey ..	1
Lexham	6
Liliput	1
Little-Sodbury ..	1
Longleat-Park ..	13
Lullington	4
Maisey-Hampton ..	2
Mareham	1
Marston, near Frome ..	1
May-Place, near, ..	2
Mazen-Hill	6
Meggot's-Mill, Coleshill,	2
Melbury	11
Meils	1
Mesterham (Well) ..	1
Minchinhampton-Common	1
Minsmere (Iron Sluice)	1
Mitford	5
——— Inn	2
Monkton-Combe ..	4

	Species.
Moushold	2
Muddiford	1
Nailsworth	1
Naunton, near, ..	1
Newborn	11
Newhaven Castlehill ..	2
Newark, N.E. of, ..	2
Northampton, N.W. of,	6
North-Cheriton ..	2
Northfleet	1
North Wilts Canal ..	27
———— County ..	1
Norton	1
———, near, ..	7
Norwich	16
Norwich, E. of, ..	4
Oldford, near Frome ..	1
Orchardleigh	5
Ormington, S.W. of, ..	1
Penard-Hill	3
Peterborough, near, ..	1
Petty-France	3
———— S.S.W. of,	7
Pewsey	3
Pickwick	14
Pipe-House	1
Playford	4
Portland-Isle	5
Pottern	1
Poulton	1
——— Quarry, Bradford	1
Prisley-Farm	3
Redlynch	3
Riegate, N. of, ..	3
Road-Lane	1
Road	5
——— (Coal-trial) ..	1
Rowley-Bottom ..	1
Rundaway-Hill ..	6
Sandford (Church-yard)	2
Sallyford	1
Scend (in Kennet and Avon Canal) ..	1
Seagry (Well) ..	1
Sheldon	10

	Species.
Sheppy Isle	3
Sherborn	16
———, W. of, ..	9
Shippon	6
Shotover-Hill	1
Shrivenham (in Wilts and Berks Canal)	1
Siddington	1
Silton Farm	4
Sleaford	5
Smallcombe-Bottom ..	1
Smitham-Bottom ..	1
Stanton, near Highworth	2
Steeple-Ashton ..	10
Steppingley-Park ..	2
——— Field ..	1
Stilton-Farm	2
———, Dun's-well ..	1
Stoford	6
Stoke	1
Stone-Farm, Yeovil ..	2
Stoney-Stratford ..	2
Stourhead	5
Stow-on-the-Wold ..	1
Stratton	1
Stunsfield	13
Suffolk (County) ..	1
Sunning-Well ..	3
Surrey (County) ..	2
Sutton	11
Swindon	12
———-Well, near Wilts and Berks Canal ..	19
Tattingstone-Park ..	6
Taverham	4
Teffont	3
Tellisford, near, ..	2
———, S.W. of, ..	4
Thames and Severn Canal	6
Thorpe-Common ..	7
Tinhead	1
Tisbury	1
Towcester	1
Trimingsby	8
Trowle	7
Tucking-Mill	36
Tytherton-Lucas ..	5
Upton	2
Vineyard-Down ..	1
Warminster, near, ..	33
Westbrook	5
Westoning	1
Westwood	4
Whitby	3
Wick-Farm	11
Wighton	1
Wilts and Berks Canal	7
Wilts (County) ..	14
Wincaston	4
———, N. of, ..	6
———, S.W. of, ..	7
Winsley	6
Woburn	1
Woodford	7
Woolverton	1
Woolwich	5
Wooton-Basset, near,	7
——— -Underedged	4
Wraxhall	1
Writhlington	2
Yarmouth, W. of, ..	4
Yeovil	7

XLVI. *Preface to "The Natural History of the Mineral Kingdom. By* John Williams, *Mineral Surveyor, F.S.S.A."*

[Concluded from p. 200.]

II. The second thing proposed to our consideration in Dr. Hutton's Theory is, the consolidation of our rocks and strata, while still under the waters of the ocean, by the heat and fusion of subterraneous fire.—Our author's doctrine of subterraneous fire,

fire, and its effects in the consolidation of the strata, by means of fusion beneath the waters of the ocean, is a singular hypothesis; but it is not altogether new.

Woodward and others have advanced the notions of central and subterraneous fires; and they also pretended to account for many of the phænomena of nature from the operations or effects of these imaginary fires: but I do not know that any of them before our author gave these fires the office of melting the earthy mass, in order to cement and consolidate our strata; though Ray conjectures, that mountains might be forced up by earthquakes, and by the flatus of volcanic fire; but none, that I know of, before the Doctor, have given this imaginary central fire the office of melting the oozy bed of the ocean, in order to reduce it by fusion into solid rocks and strata.

Our author's abilities as a naturalist, and his chemical knowledge, enable him to produce and reason upon many seeming facts to support and illustrate his hypothesis; but, unluckily for this proposition, we see in little the very same natural effects produced before our eyes without the application of visible fire, though not without the influence and effects of the elementary atmospherical fire.

There is no room to doubt, that natural chemistry is more powerful, extensive, and various than the artificial. It is difficult to limit the powers and effects of variously combined mineral liquors, in dissolving part of various fossil bodies in their natural situations, in the bowels of the earth. One thing we are sure of,—that various terrene matters are in a dissolved or fluid state, mixed with the waters which percollate the pores and cranies of our rocks and strata.

As an undeniable proof of this, we see numerous fossil bodies of various qualities and degrees of hardness formed and forming before our eyes, which are as well consolidated and cemented as if they had been fused by fire upon our author's plan of cementation; and these, not in small and inconsiderable crystallizations and stallactites, but we see considerably large concretions formed by a gradual accretion of matter deposited by water. In some places, we see caverns of various degrees of extent and magnitude, some of which are almost, and others altogether filled up by a small flow of water, depositing particles of stony matter; and the bodies so formed are afterwards consolidated, in the course of no very long time, to degrees ot strength and induration equal to any of our rocks and strata. Mines recently worked are in many places so quickly choaked up by the formation of various concretions, that we are often obliged to demolish them, to prevent their stopping up the passage altogether.

I have seen subterraneous mines or galleries, which were

worked by my direction, so filled up and choaked; and I can shew some others, which, if neglected for ten or a dozen of years, would be choaked up so effectually, and the contents would be so consolidated, that it would require an expense to open them up again, almost if not fully equal to the first. The history or natural philosophy of stony concretions is already explained in the second volume of my Essays, and need not be repeated here.

We find in many places various kinds of spar, of fluor, and of agate, formed and forming by water, depositing particles of different qualities. Some of the bodies so formed are homogeneous, and some compounded. Some of these concretions assume a fine smooth uniform texture; others exhibit, when broken, a cubic and a tabulated structure; and others again have a coarse and homely grain in the inside.

In some places, the quality of these concretions is calcareous, in others siliceous, and in many places ferruginous; and we frequently find them containing a mixture of particles of different qualities. Many of these acquire degrees of strength and hardness equal to any of our rocks and strata; and therefore we may infer, that the cementing quality is either contained in the mass of matter deposited by the water, or that it is imparted by the influence of the atmosphere.

I am much inclined to believe it is the last; and I am persuaded that the elementary fire of our atmosphere inspissates a great many fluid substances in all the three kingdoms of nature; and by penetrating their masses, and being detained and lodged there, brings them gradually to various degrees of solidity, strength, and hardness.

Now it is very observable, that the cementing matter which fills up the pores and interstices of our rocks and strata,—which connects their several parts, and promotes their solidity, strength, and induration, has the very same appearance, and is of the very same quality, as the various fossil concretions we are speaking of; but both the stony concretions and the cementing quality of the strata contain a greater variety and mixture of stony matter than we can easily enumerate or describe.

From these observations we may safely infer, that these various substances of different qualities are now in a dissolved fluid state, mixed with water. The various concretions formed by water, issuing into places accessible to the external air, justifies the inference, and proves the truth of it; and that our rocks and strata are cemented and consolidated by similar substances, is evident to our senses: but whether the cementing matter was contained in, and blended with the general composition when the strata were first formed, or was afterwards insinuated by the

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percollation of water, through the pores and cranies of the strata, I will not now inquire.

Some small veins and masses of these substances, found pure in our rocks and strata, seem to favour the supposition of the strata being cemented by the insinuation of particles, and the extraordinary induration of some of our external rocks countenances the same idea; and I have no doubt, that the elementary fire has a great share in producing every degree of solidity and induration.

Much of the cementing matter of our rocks has more of the appearance of a jelly, which is hardened by degrees from an aqueous solution, than of being produced by the fusion of fire.

These observations and facts make it evident to a demonstration, that fusion by fire is not necessary for the cementing and induration of our rocks and strata. We have abundance of examples in little of a contrary process; and, in truth, the component parts of some of our strata, the inflammable quality of others, and every situation and phænomena of the strata in general, proves, that they have not been affected by fire.

We see evident marks of water in the disposition, structure, and form, and in all the exterior and interior phænomena of the strata; but we see no real mark or character of fire, excepting in volcanoes, which are accidental, local, and very limited, have every character of being accidental, and only produce disorder and confusion; and, moreover, the origin and natural history of volcanoes is pretty well known, and is investigated and explained in the second volume of my Essays.

The philosopher or naturalist, who can deliberately embrace the idea of our real strata being cemented and consolidated by fusion by fire, either under or out of the waters of the ocean, must have his mind strangely warped by attachment to system. Such a heat as would melt and bring the whole solid globe to a state of fusion, must necessarily heat the whole waters of the ocean up to boiling, and the boiling heat of the waters must continue for many ages.

I suppose, that a solid globe of the magnitude and density of our earth, heated to a state of fusion, would require many thousand years to cool again to the temperature of our earth and water; of consequence, the waters would be kept in a boiling state the most of the time: What then would become of all the finny and testaceous tribes of the ocean?

Neither any of them, nor any of their spermatic powers and virtues could possibly live in such a heat; of course, they must be all created anew after each of these worlds is cooled. The terrestrial tribes must be in as bad a situation as those of the watery element. This appears to be an aukward hypothesis.

With respect to the solid part of the globe itself, such a subterraneous heat as would effectually penetrate the whole mass, so as to bring every part to a state of fusion, instead of leaving distinct and regular strata of various qualities, thickness, and other characters, separated from one another, as we find them, the whole solid globe must be run together into one solid slag, which might exhibit many cracks and fractures after cooling; but they would all be the cracks and fractures of an immense mass of glass or slag.

There could be no horizontal divisions, nor marks of strata of any kind, nor could we have any coal, nor any useful stone or fossil whatsoever.—Such is the nature of this extraordinary hypothesis!

We have the most early traditions of our globe suffering a great catastrophe and change by water, which is recorded by Moses, and by many other eminent ancient philosophers; and Count Buffon, Dr. Hutton, and many other modern naturalists, see and acknowledge the marks of water in all parts of the superficies of the globe: but such is their bias to the system of fire, that they attempt to convert all the rocks and strata of the globe into so many lavas of different colours and structures; and in order to countenance and assist their favourite agent, with all the powers of a heated unguarded imagination, one goes up to the source of all fire, in order to have the solid parts of our globe melted down in the sun; another goes down to the subterraneous regions, and blows up his fire there to a sufficient degree of heat to melt all the superincumbent rocks and strata to the degree of fusion, even when immersed under the waters of the ocean, which is, I think, a new method of fusing earthy matter by fire.

Others again are content to honour this agent with the formation of some few of our strata, such as the basaltes, and a few others of nearly a similar appearance; but after all that they have advanced, or can advance, to countenance this hypothesis, it is certain that none of the rocks and strata, which are a part of the solid superficies of our globe, exhibit any of the real marks and characters of being formed by fire.

The quality, component parts, interior structure, and appearance of our rocks and strata, are very distinguishable from slags or lava. Dr. Hutton acknowledges this in the 66th page of his Theory of the Earth, where he says, that "a fusible substance, or mineral composition in a fluid state, is emitted from those places of the earth, at which subterraneous fire and expansive force are manifested in those eruptive operations. In examining these emitted bodies, men of science find a character for such productions in generalizing the substance, and understand the

natural

natural constitution of those bodies. It is in this manner that such a person finding a piece of lava in any place of the earth, says with certainty, Here is a stone which had congealed from a melted state."

This passage is abundantly distinct; and I will say further, that it is generally very easy for every unprejudiced naturalist to distinguish a real stone from a piece of slag or lava. The basaltes is a real stone, which all modern philosophers have set down as belonging to the class of lavas; but I have made it evident in my Essays, that the basaltes is a real stone, the component parts of which I have pointed out; and I have made it appear, that there are in several places many and extensive strata of this stone, which are disposed in their stations among other strata of different characters and qualities, which are placed above and below the several strata of basaltes, and these strata of basaltes spread out as wide, and stretch as far every way as the other different strata among which they are ranged; and therefore, no man, who understands the real structure of the superficies of our globe, will pretend to say that basaltes is a lava, unless he says that all the other strata which accompany basaltes are also lava.

Where strata of basaltine rocks are blended promiscuously among strata of different rocks, it is necessary either to call them all strata of lava or strata of stone. Dr. Hutton indeed talks in his Theory of inserting a lava, viz. basaltes, among other strata of different qualities; but I would ask the Doctor how he is to lift up the superincumbent strata to a sufficient and equal height from the strata below them, for many miles extent every way, and to keep them asunder, until such a quantity of melted lava is poured in as will fill up all the extensive empty space to form the new inserted stratum.

I am speaking of regular and extended strata, which belongs to the natural history of basaltes, and I can shew Dr. Hutton a considerable number of strata of basaltes, blended stratum super stratum, among other various strata of different characters and qualities, among which are a considerable number of strata of pit coal; and some of these coals are in immediate contact with strata of basaltes, as the immediate roof and pavement of the coals; and I can shew him all these several strata, with their concomitant strata, in a stretch of many miles; and I can shew similar phænomena in West Lothian, in Ayrshire, and in Fife, &c.; and, therefore, it is difficult to believe that basaltes is lava, unless we also believe that seams of coal, and all their concomitant strata, likewise are lava, which sounds very like an absurdity.

It appears to me rational, and even necessary to suppose, that

if the strata were consolidated and cemented by the heat and fusion of subterraneous fire, all the strata, which have a tendency to, and may easily be hardened by fire, would be found in an indurated state; but this in fact is not the case,—so far from it, that it is well known to every person who takes the least notice of these things, that we find in all countries great numbers of tilly and argillaceous strata, so very soft, that they differ little from a mere sediment from which the water has been pressed out, and which decomposes and falls to a mere sediment or clay, almost immediately upon being exposed to the external air.

And it is remarkable, that these soft argillaceous strata are commonly situated immediately above and below very hard strata of indurated stone, upon which the external air has no sudden visible effect. How shall we account for this fact upon this hypothesis? It cannot be pretended that these soft strata contain any marks or characters of being consolidated by the heat and fusion of fire; for they are not consolidated nor cemented at all, but only compressed by the superincumbent weight of strata; nor can it be pretended, that they are not capable of being hardened by fire.

In fact, we know the contrary by experience, as they are every day hardened in our open fires, and in proper kilns, for various purposes, and to various degrees of solidity and induration. If subterraneous fire had produced the solidity of our rocks, these soft substances would have been indurated, as well as their concomitant strata.

But these soft strata are a proof, that our rocks are cemented by a terrene, sparry, and siliceous fluid, which is, by degrees, inspissated and hardened by the pressing out or evaporation of superfluous moisture; and they also prove, that these argillaceous strata can only be consolidated and cemented by fire, which has not been applied to them. We can only select a few facts which oppose this system. The instances to be found in the book of nature are endless.

III. The third proposition which we are to consider in our author's Theory of the Earth, viz. That the rocks and strata, which were formed and consolidated beneath the waters of the ocean by subterraneous fire, were afterwards inflated and forced up from under water, by the expansive force of the same subterraneous fire, to the height of our habitable earth, and of all the mountains upon the face of the globe, is an hypothesis as singular and extraordinary as the consolidation of strata beneath the waters of the ocean by the heat and fusion of fire.

Most of the operations and effects of subterraneous fire, that we have any knowledge of, are outrageously violent and destructive, and only produce disorder and ruin. If the bed of the

ocean

ocean was really to be forced up by subterraneous fire to the height of our mountains, we might expect to find as great confusion and disorder, and marks of the ruins of a world, among Dr. Hutton's mountains as among Dr. Burnet's; but I have shewed, in my Natural History of Mountains, that the strata of our real mountains are as regular as in any of the plains.

In truth, I have not seen such regularity of the strata any where else as among the highland mountains of Lochaber, which are the highest in Britain. The local examples, which I have pointed out there, will evince the truth of this assertion to any who wish to ascertain the fact.

Our author lays great stress upon the phænomena of mineral veins, and of the ores and other substances found in them, to support and confirm his fiery system: but, in truth, every appearance of mineral veins, and of their contents, point to water with a distinct and legible index, as the chief agent in their formation, &c. which subject I have investigated and explained in my Natural History of the Mineral Kingdom.

Upon the supposition of our author's Theory of Mineral Veins being true, all our veins should be wide above, and narrower below, which is not found true in experience, very many of them being exceeding strait and narrow for many fathoms next the surface, which are very wide further down; and if this Theory was true, every substance found in these veins should be the hardest in all the bowels of the earth, because the force and violence of the subterraneous fire would have a much freer passage through these open fissures, than through solid unbroken strata of several thousand miles of thickness; but this, in truth, is not the case, the inside of many of our mineral veins being exceeding soft and argillaceous.

Again, upon the supposition of the contents of our mineral veins being formed by metallic steams, forced up from below by the influence of subterraneous fire, our mineral ores should be all pure and unmixed with earthy or stony matter, which is not so; and moreover, upon this hypothesis, no metallic or mineral ore would be found out of the cavities of mineral veins; but neither is this the case; on the contrary, every mineralist knows very well, that gold, silver, copper, tin, lead, iron, &c. are commonly found, in a dispersed state, in large and smaller grains, flowers and masses, throughut the body of many of our rocks and strata, intimately mingled with their composition as one of the component parts of such rocks and strata.

Gold is generally found in grains of various sizes, mixed in the composition of many rocks and strata, and the origin of gold-dust is from the decomposition of the superficies of these rocks, which is washed down by the floods, and deposited in the beds of rivers.

Iron

Iron is blended in great quantity in the composition of most of our rocks, and so abundantly in some of them, as to be worth smelting out for use; and, moreover, we have in many places great numbers of whole strata of iron-stone so rich as to be equal, if not to exceed, the best of our iron ores in the produce of the furnace.

In working downwards, many of our mineral veins are cut out, and fail at various depths, by a different stratum coming in below, which the vein does not penetrate. The rich vein of lead at Llangunog, in Montgomeryshire, which was five yards wide of solid ore, was cut off below in this manner:

A bed of schistus came in at a certain depth below, which cut out both the ore and the vein so entirely, that no vestige of either entered the schistus, or could ever after be found. Extensive trials were made on all hands to no purpose, as neither vein nor ore ever appeared.

These circumstances do not agree with the idea of our ores being formed by mineral steams, forced up by subterraneous fires; and therefore we must acknowledge, that the substances of which our ores have been formed were poured into our veins by water from above, as well as the various spars and all the contents of mineral veins.

There is a curious and surprising mixture of many different substances in several mineral veins. In some of them, we find lead, copper, silver, and several other metallic and semi-metallic ores; and, in the same vein, we find calcareous and siliceous spar, with a variety of other stones and mineral matters of various colours, qualities, and degrees of hardness; and we frequently find many of these, and sometimes all of them, blended together in the concavity of the same vein.

Every phænomenon of these different ores and different stones proves to ocular demonstration, that all the different substances in the composition were poured in from above, and mixed together while in a humid or fluid state, and that they were afterwards consolidated together into such compound masses as we find them.

IV. The fourth proposition offered to our consideration, in our author's Theory of the Earth, is also pretty singular, which is, that these operations of nature, viz. the decay and waste of the old land, the forming and consolidation of new land under the waters of the ocean, and the change of the strata now forming under water into future dry land, is a progressive work of nature, which always did, and always will go on, forming world after world in perpetual succession.

This hypothesis agrees pretty nearly with Count Buffon's, only that the Count brings about his successive changes by a

watery

watery process, without the agency of fire, after having the original matter* of the whole globe once thoroughly vitrified in the sun.

Both the Count and our author strenuously insist upon the waste of the superficies of the mountains, and of the rocky shores of the ocean, by the force of the tides and storms, as an infallible proof of the gradual destruction of the existing dry land, and they both infer from hence the successive changes of habitable worlds as a necessary consequence.

I have in my Essays fully investigated and explained these matters. I have pointed out the utmost extent of the waste of the mountains; and I have acknowledged, that the weight of mighty waves, propelled by the tides and stormy winds, have powerful effects in undermining and wasting the rocky shores; but then I have made it evident, that this waste and destruction only advance to a certain length and degree, where it stops; and I have drawn the line, and pointed out the depredations of the waves with some exactness; and have made it evident to our senses, that hitherto they come, but no farther.

In some places, the sands are interposed to defend the rocks, and the very slow diminution of the sands by attrition is abundantly made up by fresh supplies furnished by the rivers. In other places, the rocks are covered by a shelly incrustation, the work of small testaceous tribes, which perfectly defends these rocks against any injury from the waves.

We may suppose, that all or most of our maritime coasts were at first exposed to the ravages of the ocean. At present, the greatest part is defended by the sands and testaceous incrustations; and it is rational to suppose, that, in the course of time, all the shores of the ocean will be perfectly defended by these means.

With respect to the real encroachments which the sea has hitherto made, or may hereafter make, upon the land, I think we may safely conclude, that a million of acres of new land have been made from the sediment of the rivers for every single acre of the rocky shores that has been wasted by the waves of the sea.

This is no supposition; it is a fact abundantly evident to our senses; and it is a sort of retrograde operation towards the successive change of worlds contended for by our philosophers.

Dr. Hutton investigates a considerable number of fossil bodies, and explains their phænomena to countenance his own hypothesis. It would extend this preface to too great length, were I to examine what he has advanced upon them all.

At present, I will only take notice of the testaceous tribes of the ocean. He tells us, that these exuviæ, being found in the body

body and composition of our rocks and strata, is a clear proof, that those strata were formed by water,—which is so far true. I also assert, that these exuviæ, and all the other remains of plants and animals found in the body and composition of our strata, is a decisive proof that the strata were formed by the agency of water; at the same time, I positively deny that our strata were formed beneath the waters of the ocean.

The natural history of the formation of our strata is fully explained in the second volume of my Essays upon rational and mechanical principles, to which I refer for satisfaction on this topic. In my opinion, our author's philosophy is not more exceptionable in any part of his Theory than in treating of marine testaceous animals, as he makes these in effect to be very extensive creators of matter, which is exalting them much too high in our system of things.

The Doctor says, that one-fourth part of the solid bulk of our globe is composed of limestone, marble, and other calcareous matter, which I think is giving it too great a proportion. My general observations have been pretty extensive; and, as far as I can judge, all our limestones, marbles, chalk-stone, and clay-marl, which is soft limestone, and all other calcareous fossil substances, may amount to about a seventh or eighth part of the solid bulk of the superficies of the globe, which is a great deal indeed.

Now our author asserts, in plain terms, and in several parts of his Theory, that this immense bulk of solid calcareous fossil matter was all of it produced from the remains of the testaceous tribes of the ocean. In my opinion, the proposition may be reversed; and we may with more truth assert, that the calcareous matter produced them, than that they produced it.

Snail-shells are found in great numbers near old stone and lime walls; yet we never imagine that these walls were produced by snails. It is almost evident to our senses, that these animals find the calcareous matter in a fluid state mixed in the waters of the ocean and the land, which they collect and use to make shells, coral, &c. To say that they produce this matter, is much the same as to say that they create it.

Matter is only changed from one form of existence to another in the reproduction and growth of animal and vegetable bodies, but they really produce no part of matter that did not exist before in another form.

I grant, that the exuviæ of testaceous animals are found in great abundance in many of our limestones and marbles, but not in all of them. There are very extensive rocks and strata of the mountain-limestones, and marbles of various colours, texture, and degrees of hardness, in which not the least particle of shell or coral is to be found.

These

These shells are also found in several other strata besides the calcareous; all which only proves, that these marine exuviæ were blended in the mass of chaotic matter when these several strata were formed; but to say that these animals can produce any particle of matter, is not good philosophy.

We know that calcareous matter certainly exists in a dissolved fluid state, mixed in abundance with the waters of the ocean, which is separated from the water in considerable quantity, in the common process of making salt of sea brine. How the testaceous tribes make use of it in making shells and corals, is too nice a process for my investigation.

Shells and corals could not exist, as we find them in the body of the rocks and strata, upon the supposition of these rocks being consolidated by the heat and fusion of fire; because a smaller degree of heat than is sufficient to bring our rocks to a state of fusion, would calcine all the shells and corals, with the limestones to boot; and when once they are calcined, they are no more shells, &c. but quicklime, to which they would fall with the least humidity; and the whole bowels of the earth, as far as we penetrate, is full of humidity.

In short, few of our author's conclusions are defensible,—and no wonder, when he warps and strains every thing to support an unaccountable system, viz. the eternity of the world; which strange notion is the furthest of all from being defensible.

All parts of nature, the minute as well as the grand and magnificent, proclaim aloud, and point out in legible characters the infinite power and skill of the all-wise and benevolent Creator and Preserver of the universe. The Supreme Being hath highly favoured us with an exalted station, and hath given us the image of his own attributes. We daily enjoy the fruits of his care and benevolence, and we feel the effects of his goodness, whether we advert to and acknowledge it or not.

The impressions of divinity are legibly stamped on all the works of God; and when we clearly behold the characters of ineffable wisdom in the great plan of creation,—of infinite skill and intelligence in the contrivance, disposition, and fine fabric of all the parts of nature,—of almighty power in producing all things and upholding them,—and of exuberant and unbounded goodness in communicating good to all animated nature, we then have exalted ideas of the Supreme Being; and if we reflect upon our own distinguished rank and situation in the scale of beings, and of our privileges and powers of acquiring knowledge and promoting mutual and social happiness, our hearts will exult in the display of the glory of the Creator in his works; and if we believe that the Creator and Governor of the would protects and

and cares for us, our hearts will overflow with grateful love of the Deity; we shall then rejoice in his works and in his goodness.

But sceptical notions have a pernicious influence in damping the sacred fire in our hearts, in cooling the ardour of our spirits, and in blotting out the native impressions of the Deity stamped on our hearts. The wild and unnatural notion of the eternity of the world leads first to scepticism, and at last to downright infidelity and atheism.

If once we entertain a firm persuasion that the world is eternal, and can go on of itself in the reproduction and progressive vicissitude of things, we may then suppose that there is no use for the interposition of a governing power; and because we do not see the Supreme Being with our bodily eyes, we depose the almighty Creator and Governor of the universe from his office, and instead of divine providence, we commit the care of all things to blind chance.

Like a mob, who think they can do well enough without legal restraints, depose and slay their magistrates. But this is rebellion against lawful authority, which must soon end in anarchy, confusion, and misery,—and so does our intellectual rebellion. How degrading is infidelity! how miserable must a thinking man be in distress, who does not believe that there is at the head of the creation, a good, intelligent, and powerful being, who cares for his welfare through all the stages of existence!

That Dr. Hutton aims at establishing the belief of the eternity of the world, is evident from the whole drift of his system, and from his own words, for he concludes his singular theory with these singular expressions: "Having, in the natural history of the earth, seen a succession of worlds, we may from this conclude, that there is a system in nature, in like manner as from seeing the revolutions of the planets, it is concluded that there is a system by which they are intended to continue those revolutions. But if the succession of worlds is established in the system of nature, it is in vain to look for any thing higher in the origin of the earth. The result, therefore, of our present inquiry is, that we find no vestige of a beginning,—no prospect of an end."

Thus, our modern philosophers labour hard to confirm their favourite scepticism, &c. by all possible means; or, in other words, they labour hard to rob us of our best inheritance, both here and hereafter,—to sap the foundations of our belief in revelation, and of the superintending care and love, and of the over-ruling providence of the all-benevolent, all-powerful God, our Saviour, who cares for us, and upholds us through all the

stages

stages of our existence,—and like actual robbers, these philosophers give us nothing in exchange for our natural inheritance.

If they say that we are poor mistaken ignorants, and that they wish to convince us of our error,—this is worse than nothing. If we err, in charity let us live and die in this error. It is more happy to live in a full persuasion,—in a feeling sense of the love of God and man, while here, and in the confident hope of eternal felicity hereafter, than to suppose that there is no such thing,—that these divine faculties and propensities of our souls which make us capable of loving God and man,—of admiring God in his works, and of ranging through his creation with sublime delight,—shall perish for ever, and sink into the horrible gulph of *non-entity*.—Let us turn our eyes from the horrid abyss, and stretch out our hands, and cry, Save, Lord, or we perish!

XLVII. *Answer to the Letter of C. of Exeter on Steam-Boats to be used in conveying Merchandise by Sea. By Mr.* James Dawson.

To Mr. Tilloch.

Sir,—Your Correspondent C. (of Exeter) has solicited information relative to his proposed plan of constructing steam-vessels to convey merchandise between London and Exeter, with so much candour and good sense, that I cannot refrain from offering a few remarks on the subject to his consideration.—The utility and advantage of employing steam-packets, *on rivers*, to convey passengers is now pretty generally admitted; and notwithstanding that some accidents have occurred, their number is increasing on all rivers suitable for them. The speed and excellence of our coach continue however formidable rivals to them. The conveyance of *merchandise on rivers* has latterly become an object of interest. In Scotland one or two vessels are used for that purpose. In America, where the rivers are deep, broad, and navigable for several hundred miles, and wood for fuel cheaply procured, several steam-vessels of great dimensions, with powerful engines aboard, are advantageously employed in conveying *merchandise* as well as passengers.

Stimulated by the success attendant on these first efforts—the Americans have even gone much further. Possessing a country abounding in timber, they have constructed frigates and floating batteries impelled by wheels worked by steam. These attempts however have not, nor cannot, succeed to any valuable extent, as long as wheels are the medium of action on the water—because, as their action is necessarily limited and superficial, they must

move

move bodies deeply immersed to great disadvantage. A few years ago a steam-packet, of which I was a part-owner, having made a successful voyage *by sea* from Scotland to London, led the way to similar attempts, and finally to the establishment of the steam-packets to Margate. The above vessel plied some time on the Thames, and subsequently passed over to the Seine. I confess, however, that I am decidedly of opinion, that as long as the common rotatory impellers are employed, such steam-packets are infinitely more unsafe *at sea* than vessels impelled by wind. As coasters, less risk is of course incurred; because in case of accident, as steam-packets draw little water, they may run ashore with safety. Necessity has compelled many persons to make voyages by sea in open boats, and they have frequently escaped; but I believe few people would prefer from choice that mode of conveyance. Deceived by some exaggerated statements and reports (and contrary to my opinion detailed at some length in the newspapers of the day), a most respectable company in Dublin undertook the conveyance of *passengers* by steam-packets with wheels, between Holyhead and Dublin. The attempt, however praiseworthy, has not repaid the spirited proprietors the many thousands they have expended therein.

The conveyance of *merchandise by sea* from Scotland to the North of Ireland was attempted by steam-vessels worked by ordinary wheels:—but I presume a failure, as I do not hear of their continuing to ply. If the secure conveyance of passengers *by sea* in steam-packets involves difficulties on the known plan, it is clear that the safe conveyance of *merchandise* involves greater. A knowledge of the difficulties to be surmounted is, however, a great step towards finding the means of overcoming them.

At first eight-horse engines were employed in boats. Mechanics, accustomed to machinery acting on immoveable fulcrums, and perhaps ignorant of the laws of fluids, imagined that they had only to increase the power of their engines, and that thereby the velocity of the vessel would be increased in proportion: but although thirty-six- and forty-horse engines are now in common use, little comparative advantage has been derived therefrom; and wherever the power is expended in giving an undue velocity to the impelling wheels, much water is lifted, and the speed of the vessel is diminished. In short, the waste of power, owing to the imperfect leverage on the water of the wheels in common use, is enormous.—Still *on rivers* (as nothing superior has appeared in use) this imperfection, being resolvable into a mere question of expense and convenience, forms no insurmountable bar to their beneficial employment. It should always be held in view, that large engines are expensive, are weighty, occupy much valuable room, and consume daily large quantities of fuel, oil, &c. increase the

the immersion of the vessel, and require it to be built of strong and ponderous materials; and after all, if a rope was attached to the stem of the vessel, and a weight equal to *one-eighth* the full power of the engine was fixed to the other end and passed over a pulley, the vessel would be drawn faster through the water than it could be impelled by the engine moving wheels;—it therefore is a great desideratum to obtain an action on the water at once convenient in its application, and producing an effect equivalent to the moving power, which under such circumstances might be materially reduced.

Aware of the danger, waste of power, and inefficiency of wheels to move vessels deeply immersed *at sea*, I devised many substitutes for them; but what I give a preference to, is a subaquatic lever, simple as the common oar; but which, owing to the adoption of a novel principle in its construction, possesses far greater power on the water; and which, when moved either up and down or to and fro therein, will communicate an unceasing forward motion to a body.

I conceive it superior to the common oar: 1st, In power: 2dly, in being applicable with effect to the largest vessels *at sea:* 3dly, in not requiring to be feathered: 4thly, in not losing time in rising out of the water. I conceive it superior to wheels in simplicity, possessing a better and equally unceasing action on the water, and far more convenient and secure in application, while its power of leverage may be increased almost *ad infinitum*. Wheels cannot be multiplied or enlarged with corresponding effect or convenience; but a simple reciprocating lever, such as above described, may; because, like the feather in a wing, it will prevent little surface in the line of motion. A body wholly immersed in water is equally pressed and supported throughout, and therefore is not so liable to be broken as a wheel posited in air and water is. I confess I have not as yet had an opportunity of trying this new species of lever on a large scale, I therefore naturally feel diffident in offering it to public notice: but I will show a model of it at work to any scientific gentleman, and explain its peculiar properties and application to any person seriously inclined to adopt the use of it. Except in diminishing the weight of the steam-engines used in vessels, and dismissing the fly wheel, I know of no valuable improvement that has taken place in steam-boats since they came into use. The cause I apprehend is, the vast expense of experiments in this line, and the very limited knowledge we possess of the laws and properties of fluids. The House of Commons (the safety-valves of the purse of the nation) has humanely attempted to legislate for steam-boats, but has offered no rewards for their improvement. Since then, I have heard that the owners of a steam-packet on the

Thames advertised perfect security to their *passengers* (under the new law) in the morning, and kept their word by blowing up in the evening, when only *the crew* were injured.

Your correspondent has fairly stated the advantages likely to result from his plan of placing the wheels of his proposed steam-vessel at the stern;—but practically I fear he will find it subject so some disadvantages. In the first place, wheels placed at the stern will not impel a vessel with equal effect with wheels placed on each side. 2dly, Whenever the wind is strong on either bow the head of the vessel will not at times obey the helm, with the due action of which wheels at the stern are likely to interfere. 3dly, If the wind is strong and fair, the pitching of the vessel and the roll of the sea aft, will more seriously disturb the action of the wheels than if they were placed at or near midships. Presuming that it is intended to use the wheels in ordinary use, it would be easy for me to prove the truth of the foregoing observation;—but I wish to be brief, and avoid detail as much as possible. My object is not to damp the spirit of enterprise, but to direct it as far as the case will admit into a safe and profitable channel; and if your correspondent is determined to follow up his plan of using two wheels at the stern, I would beg leave to recommend him a particular construction of wheels, which I invented and used many years ago, and which will materially assist his purpose and obviate in a great degree the objections I have started. The paddles of the wheels I allude to when at rest, present their edges in a line with the keel of the vessel; of course they expose little surface to the direct action of the wind or sea. When made to revolve, a simple but effectual contrivance obliges each paddle as it successively enters the water to gradually present its full surface thereto, and consequently to rise out of the water on its edge; each paddle may therefore be considered a vertical rotatory feathering oar, free from all shock in entering the water, obtaining the full effect therefrom, and rising out without any lift thereof. But as the degree of obliquity of each, and every paddle, may be varied in a moment at pleasure; it follows, that a vessel may be both *impelled* and *steered* by such wheels; and in place of the horizontal rudder, these rotatory impelling rudders might be used with advantage in every steam-boat, either as a substitute for, or in addition to, wheels placed at the sides. When so posited, the general arbor, instead of projecting as customary three or four feet on each side of the vessel, need only project half that space; and as there is no lift of water, no casing is necessary over them, the mechanism is boxed in and secure from all external injury, is not liable to be deranged, and works with very little friction.

A little reflection will show that these rotatory rudders might furnish

furnish the means of directing a steam-vessel to any given point, even in opposition to a moderate wind and tide, without the aid of a man aboard, for a limited time:—if therefore such a vessel had been fitted up as a fire-ship, the Algerine navy might have been destroyed without the loss of a man on our part.

On some future occasion I may, perhaps, send you some observations on the subject of towing vessels by means of steam-boats; On the best forms to give steam-vessels intended for the sea; On the utility of a change likewise in the form of sailing-vessels; On the practicability of employing steam as a moving power aboard vessels without the possibility of an explosion:—but for the present I feel I have already trespassed too much on your valuable space; and therefore remain

Yours, &c.

4, George's Place, Dublin, Sept. 14, 1817. JAMES DAWSON.

XLVIII. *On the Cause of the Changes of Colour in Mineral Cameleon.* By M. CHEVREUL*.

1. SINCE the time of the illustrious Scheele many important facts have been added to the history of manganese; but no person, to my knowledge, has made any particular inquiry into the cause of the changes of colour exhibited by mineral cameleon †. I will endeavour in this note to deduce from observations of my own, an explanation which, if it is admitted, will be susceptible of many new applications.

2. I must begin by stating the properties which Scheele has recognised in mineral cameleon. 1. On the solution of cameleon in water, a deposition of a fine yellow powder takes place, and the liquor passes insensibly to a blue colour. Scheele believes that the yellow powder consists chiefly of the oxide of iron; that the blue is the true colour of the cameleon, and is only changed when iron is in conjunction. 2. Cameleon mixed in water becomes decomposed; the mixture appears violet, then red; and when the red particles combine, the red colour disappears and the deposit of cameleon presents nothing more than the natural colour of the oxide of manganese. 3. Lastly, the same effect takes place when a few drops of acid are added to the solution, or when it is exposed for some days to the open air: in this last case the alkali unites itself to the carbonic acid of the atmosphere. Let us now pass to the facts which I have observed.

* From a work on Manganese, by M. Chevreul.

† The substance so called is a combination of potash with an oxide of manganese.

3. I have prepared the cameleon of which I have made use, by exposing in a crucible of platinum to the action of a red heat for twenty minutes, a mixture of a *gramme* (about a scruple) of oxide, red-brown, obtained by the calcination of the carbonate of pure manganese with eight *grammes* of potash. The green mass produced by this operation was twelve hours afterwards immersed in water. Whatever was the proportion of water employed, there was always a large enough quantity of the oxide which did not dissolve. I do not think that the whole of the oxide has ever been separated by the action of the water; I believe that there is a portion of it, which, after being incorporated with the alkali, separates itself from it upon the solidification of the cameleon by cooling. This last portion appears often under the form of little brilliant spangles, similar to the sulphuret of molybdenum.

4. When the cameleon dissolved in water passes to blue, it is not by depositing from the oxide of iron yellow; for cameleon which has been prepared with the pure oxide of manganese yields a similar deposit, and the liquid when perfectly clear, being evaporated to dryness, leaves a residue, *which takes, when it is exposed to a red heat, a beautiful green colour, and communicates the same to water when immersed in it*. Now, if the colour of cameleon was naturally blue, it ought to be obtained of that colour, upon dissolving with potash the oxide which has been deprived of its pretended oxide of iron. Either then the colour of cameleon is not blue, or the observation of Scheele is not proved.

5. When cameleon passes more or less slowly from green to red, it presents a series of colours in the order of the iris; viz. green, blue, violet, indigo, purple, red. Not only cold water, but even carbonic acid, carbonate of potash, subcarbonate of ammonia, and lastly hot water, when added to cameleon, produce these colours. It is observed that the latter even produce them with more rapidity than cold water.

6. According as it appears to me, the green solution of cameleon is the combination of caustic potash with the oxide of manganese, and the solution which becomes red by carbonic acid (of which alone I at present speak) is a triple combination of potash, the oxide of manganese, and carbonic acid. It may be also necessary to take account of the water which holds these combinations in solution: but the proportion of water does not seem to me to have any sensible influence on their coloration; for if we saturate with carbonic gas, a green solution, formed of one part of cameleon and ten parts of water, it will pass to red, depositing at the same time a little of the oxide; then on putting into this red liquor some dry caustic potash it resumes the green colour;

colour; and afterwards, on saturating the alkali added by the carbonic gas, the red colour is reproduced, accompanied with a deposition of a little of the oxide. In the last place, I have observed that precipitating by the water of barytes a part of the carbonic acid from a red solution of cameleon, changes it into green cameleon*.

7. Cameleons which become blue, violet, indigo, and purple, by the action of carbonic acid, appear to me to be mixtures of green and red cameleon. In proportion, accordingly, as we add more and more considerable quantities of green cameleon, we obtain *successively* purple, indigo, violet, and blue liquids. It is easy from this to conceive, how by adding at intervals to a green cameleon some small quantities of carbonic acid or carbonate of potash, blue, violet, indigo, and purple liquids will be obtained; and again, how the liquids may be obtained in the inverse series, by adding, at intervals, to a red cameleon small quantities of potash.

8. Let us now endeavour to prove by analysis the nature of the intermediate cameleons between green and red. If we filter some green cameleon a certain number of times upon a filter† of sufficient size, the cameleon will be decomposed into potash, which will remain in the water, and into oxide of manganese of a brownish yellow, which will attach itself to the slips of paper, in virtue of an affinity analogous to that which occasions the combination of cloths with the mordants employed in dyeing. A similar decomposition will take place, if we introduce a piece of paper into a solution of green cameleon, separated from all contact with the air;—the results are the same with red cameleon. The chemical action of paper on solutions of cameleon being thus demonstrated, the possibility may be conceived of reducing by filtration a liquor containing the two cameleons to a simple solution of one of them, provided there exists always a difference in the tendency which the oxide of manganese of the green combination and the carbonated combination have to unite with the paper; and so in fact we find the case to be: for if we filter blue, violet, indigo, and purple cameleons, the red cameleon is decomposed, while the green cameleon passes to the side of the filter.

9. The preceding explanation is applicable to changes pro-

* It is not necessary to use as much of the barytes as will saturate all the carbonic acid; for it would precipitate with it a rose-lilac combination of the oxide of manganese and barytes. This combination, which is a species of cameleon, may perhaps be spoiled by the subinixture of acetic acid of carbonate, which there is no doubt exists in compounds of this sort.

† Which ought to be washed with hydrochloric acid, to prevent any foreign matters from attaching to the slips of paper.

duced by the subcarbonate of ammonia and the carbonate of potash;—but is it equally so to the changes produced by distilled water? I do not think it is, although indeed the purest water which I have been able to obtain has always presented some sensible quantity of carbonic acid, or of subcarbonate of ammonia. Thus much I can affirm, that the intermediary cameleons produced by water are invariably formed of green cameleon and a red liquor; for all of them become green after being filtered, and when potash is added are converted into green cameleons. What proves, besides, that the carbonic acid has no influence on the colour of the red liquor of these cameleons is, that water which has been reduced by boiling to a fifth of its volume, and which ought to contain less carbonic acid than cold water which has not been boiled, being mixed when hot with green cameleon, reddens it much more rapidly than cold water: and again, that when a little more hydrate of barytes is added to boiling water than is necessary to precipitate all the carbonic acid contained in the water, if it is afterwards turned into green cameleon, it will change to red, *although the carbonic acid has been wholly extracted.* Is it not possible that this red colour may be the result of an action of the potash upon the oxide less strong than that exercised by the same alkali upon the oxide of green cameleon? And is it not also possible, when carbonic acid is present, that it may have the effect of weakening the action of the potash?

10. The oxide of green cameleon possesses without doubt the same degree of oxidation as the oxide of red cameleon, and that oxide contains more oxygen than that of salts of manganese uncoloured; so that on heating hydrochloric acid with green or red cameleon, the former disengages itself from the chlorine, and the latter becomes discoloured. Scheele has before remarked, that a great number of matters susceptible of absorbing oxygen produce the same effect of discoloration as hydrochloric acid. But it may be asked, Does the cameleon contain the natural oxide, or the oxide which is produced by exposing the latter to the action of fire? If we consider the impossibility of uniting the first to acids without subjecting it to a previous deoxidation; that cameleon supersaturated with sulphuric, nitric, and other acids forms red salts, in the same manner as the second of the oxides referred to; and further, that carbonic acid reddens green cameleon without producing any effervescence,—it would seem to follow that the oxide of cameleon is less oxidated than the natural oxide. I have made several experiments to ascertain the correctness of this conclusion. I heated in a stone jar 25 grammes of the oxide of native manganese with 200 grammes of potash *à l'alcool;*

à l'alcool; I collected from the water a little azote, with carbonic and inflammable acid; which last indicated that an alcoholic matter remained with the alkali. The jar was speedily penetrated by the potash. I repeated the same experiment with potash *à la chaux*. I did not obtain any inflammable gas; but the jar was penetrated as in the preceding experiment. The cameleon of the first experiment was green; but when diluted in water it did not yield a permanently coloured dissolution. The cameleon of the second experiment, being put into water, did not disengage any remarkable quantity of oxygen; the liquor which it yielded was of a permanent green; heated by mercury without the contact of the air, it became discoloured without presenting any of the colours of the series; but when carbonic acid was added it presented the whole series. In order to prevent the corrosive action of the potash upon the jar, I made another experiment, in which I heated 30 gr. of oxide with 270 gr. of carbonate of potash which had been reduced in a great measure by the heat into subcarbonate. The jar was not in this instance affected, and the result I obtained was a mixture of about two volumes of carbonic acid and one of oxygen. The cameleon produced was of a greenish blue; put into water, it precipitated a good deal of the oxide, of which part was micaceous and part was dissolved, and imparted a green colour to the water; but this solution lost its colour so quickly, and was besides so slightly charged with oxide, in comparison to the quantity which had been heated, that I do not regard this experiment as absolutely conclusive of the supposition, that the native oxide of manganese loses oxygen on uniting itself to potash—though it certainly renders it very probable.

11. If the explanation which we have given of the colours of cameleon is exact, is it not probable that some minerals may be enamelled with blue, with violet, and with purple, by green and red combinations of manganese? Is it not probable that the alkaline substances, earthy or vitreous, which become tinged with red by the oxide of manganese, exercise upon it the same action as the acids? And may not a combination of this sort along with a green alkaline combination of the same oxide, form mixtures of colours analogous to blue, violet, indigo, and purple cameleons? In short, does there not seem some analogy as to chemical action between the oxide of manganese and certain vegetable colouring principles, which become green by the alkalies, and red by the acids?

XLIX. *On an apparently new Species of Wren, discovered at Tunbridge Wells, by* Thomas Forster, *Esq. F.L.S.*

To Mr. Tilloch.

Sir, — I beg leave to communicate the discovery of what seems to me to be a new species of wren, which I have of late seen in the neighbourhood of Tunbridge Wells. I saw it in the month of September and early in the present month, among the trees, particularly the firs, pines, and willows. It was about four inches and a quarter long, and bore the nearest resemblance in form to the smallest willow wren, *Sylvia Hippolais* of Latham and E. Forster's catalogue. But it differed in colour: the upper parts of the whole body, head, wings, and tail being of a pure dark brown: the under parts silvery white. This may possibly be only a variety of the *Sylvia Hippolais*, as birds of this kind vary extremely; but if it be a distinct species, both its form and manners place it among the *Sylviæ:* and I should propose to call it *Sylvia brunnea*. It nearly answers to the description of a bird which Dr. Leach (of the British Museum) calls *Curruca*, of which he has spoken to me as being a new wren.

We have all the three known species of willow wren at Tunbridge Wells; and I have observed a considerable variation of the plumage in all of them, which has, no doubt, been in part the cause of the great confusion found in the descriptions of birds of this genus among naturalists. I proceed to enumerate some of the most common varieties I have noticed.

Sylvia Sylvicola; or the largest willow wren. This, which somewhat exceeds the common willow wren in size, is found with the following varieties:

α. With the upper parts greyish; the under parts almost white.

β. The upper parts yellowish, green mixed with dusky; and the under parts yellow, more or less deep.

γ. Almost yellow like a Canary-bird, there being only a few dusky specks on the wings, and dusky quills. I have seen this variety in the garden of Mrs. Forster, of Walthamstow, on the spruce fir-tree.

Sylvia Trochilus, the middle willow wren, varies as follows:

α. Greenish ash-colour above, and white with a tinge of yellow beneath.

β. Greenish olive, mixed with yellow above, and deep yellow on all the under parts. This seems to be the first-year's bird; and the plumage changes afterwards.

Sylvia Hippolais, the least willow wren. This varies only in the

the lighter or darker shades of its plumage, and has less yellow in it than any other species. I have purposely given only the ostensible varieties of these birds, which may be seen when the bird is on the trees.—The new wren may perhaps become the subject of future observations of a more detailed and accurate nature.

I am, &c.

Walthamstow, Oct. 16, 1817. THOMAS FORSTER.

L. *Notices respecting New Books.*

An Experimental Inquiry into the Laws of the Vital Functions; with some Observations on the Nature and Treatment of Internal Diseases. By A. P. WILSON PHILIP, *M.D. F.R.S.E.*

[Continued from p. 228.]

"THE spasmodic asthma is fortunately a very rare disease; so much so, that but one case of it has occurred to me since I have employed galvanism in asthma, while I have had an opportunity of employing this remedy in about forty cases of the habitual form of the disease. I cannot therefore, from experience, speak with certainty of the effect of galvanism in the former. In the above case it was twice employed in the paroxysm, and I could observe no relief from it. In both instances the patient said that, had it not been used, the symptoms would have been more severe. In this patient, the spasmodic paroxysm was often succeeded by a state of habitual asthma for several weeks, in which galvanism gave immediate, but temporary relief.

"Of the above cases of habitual asthma, many occurred in work-people of the town where I reside, who had been obliged to abandon their employments in consequence of it, and some of them, from its long continuance, without any hope of returning to regular work. Most of them had tried the usual means in vain. By the use of galvanism they were relieved in different degrees, but all sufficiently to be restored to their employments. I have seen several of them lately, who, although they have not used the galvanism for some months, said they had continued to work without any inconvenience. Some, in whom the disease had been wholly removed, remain quite free from it; some have had a return of it, and have derived the same advantage from the galvanism as at first.

"I have confined the application of galvanism to asthmatic dyspnœa. I think there is reason to believe, from the experiments which have been laid before the reader, that in inflammatory cases it would be injurious, and, in cases arising from dropsy, or

or any other mechanical impediment, little or nothing, it is evident, is to be expected from it. Habitual asthma is often attended with a languid state of the biliary system, and some fullness and tenderness on pressure near the pit of the stomach. If the last is considerable, it must be relieved previous to the use of the galvanism. In a paper which the Medico-Chirurgical Society did me the honour to publish in the seventh volume of their Transactions, I have endeavoured to show that a species of pulmonary consumption arises from a disease of the digestive organs. Many of the observations there made apply to certain cases of asthma*; I believe to cases of every species of this disease, but particularly of that we are here considering. Many cases of habitual asthma will yield to the means recommended in the above paper; but I have learned, from a pretty extensive experience, that a large majority of such cases will resist them, yet readily admit of relief from galvanism. If there is little tendency to inflammation, galvanism seems also to be a means of relieving the affection of the digestive organs. I have repeatedly seen from it the same effect on the biliary system which arises from calomel; a copious bilious discharge from the bowels coming on within a few hours after its employment. This seldom happens except where there appears to have been a failure in the secreting power of the liver, or a defective action in the gall tubes.

"I have not found that the presence even of a severe cough, which is common in habitual asthma, in which there is always more or less cough, counter-indicates the use of galvanism. The cough under its use generally becomes less frequent in proportion as the accumulation of phlegm in the lungs is prevented; but it seems to have no direct effect in allaying it. In some cases the cough continued troublesome after the dyspnœa had disappeared. Galvanism never appeared to increase it, except when the inflammatory diathesis was considerable. In some labouring under the most chronic forms of phthisis, in whom the symptoms had lasted several years and habitual asthma had supervened, the relief obtained from galvanism was very great, notwithstanding some admixture of a pus-like substance in what was expectorated. I need hardly add, after what has been said, that in ordinary cases of phthisis nothing could be more improper than the use of galvanism. The dyspnœa arising from phthisis and that from habitual asthma are easily distinguished. The former is less variable. It is generally increased by the exacerbations of the fever, and always by exercise. When the patient is still and cool, except in the last stages of phthisis, his breathing is

* See the observations on the state of these organs in asthma, in Dr. Bree's work on this disease.

generally

generally pretty easy. The latter is worst at particular times of the day, and frequently becomes better and worse without any evident cause. At the times when it is better the patient can often use exercise without materially increasing it. Changes of the weather influence it much. It is particularly apt to be increased by close and foggy weather. Phthisical dyspnœa is seldom much influenced by changes of the weather, except they increase the inflammatory tendency.

"When there is a considerable tendency to inflammation in habitual asthma, the repeated application of galvanism sometimes increases it so much, that the use of this influence no longer gives relief, till the inflammatory tendency is subdued by local blood-letting. It always gave relief most readily, and the relief was generally most permanent in those cases which were least complicated with other diseases, the chief complaint being a sense of tightness across the region of the stomach, impeding the breathing. The patients said, that the sense of tightness gradually abated while they were under the influence of the galvanism, and that as this happened their breathing became free. The abatement of the tightness was often attended with a sense of warmth in the stomach, which seemed to come in its place. This sensation was most frequently felt when the negative wire was applied near the pit of the stomach, but the relief did not seem less when it was not felt.

"With respect to the continuance of the relief obtained by galvanism, it was different in different cases; in the most severe cases it did not last so long as in those where the symptoms were slighter, though of equal continuance. This observation, however, did not universally apply. When the patient was galvanised in the morning, he generally felt its good effects more or less till next morning. In almost all, the repetition of the galvanism gradually increased the degree of permanent relief. Its increase was much more rapid in some cases than in others. The permanency of the good effects of galvanism in the disease before us, has appeared very remarkably in several cases where the symptoms, after having been removed by it, were renewed after intervals of different duration by cold or other causes. In these cases means which, previous to the use of galvanism, had failed to give relief, were now successful without its aid; or with few applications of it, compared with those which had been necessary in the first instance. I have not yet seen any case, in which galvanism had been of considerable advantage, where its good effects appeared to have been wholly lost. It is now about a year and a half since I first employed it in habitual asthma. Taking cold and the excessive use of fermented liquors have been the principal causes of relapse.

"The

"The galvanism was seldom used more than once a day. In some of the more severe cases it was used morning and evening. About a sixth part of those who have used it appear, as far as we yet know, to have obtained a radical cure. It in no case failed to give more or less relief, provided there was little inflammatory tendency. It failed to give considerable relief only in about one-tenth; I may add, that were it only the means of present relief, we have reason to believe that, as being more innocent, it would be found preferable to the heating, spirituous, and soporific, medicines, which are so constantly employed in this disease.

"As it often happened that a very small Galvanic power, that of not more than from four to six four-inch double plates, relieved the dyspnœa, may we not hope, that a Galvanic apparatus may be constructed, which can be worn by the patient, of sufficient power to prevent its recurrence in some of the cases in which the occasional use of the remedy does not produce a radical cure?

"I wished to try if the impression on the mind, in the employment of galvanism, has any share in the relief obtained from it. I had not at this time seen its effects in apoplexy. I found that by scratching the skin with the sharp end of a wire, I could produce a sensation so similar to that excited by galvanism, that those who had most frequently been subjected to this influence were deceived by it. By this method, and arranging the trough, pieces of metal, &c. as usual, I deceived several who had formerly received relief from galvanism, and also several who had not yet used it. All of them said that they experienced no relief from what I did. Without allowing them to rise, I substituted for this process the real application of galvanism, merely by immersing in the trough one end of the wire with which I had scratched the nape of the neck, the wire at the pit of the stomach having been all the time applied as usual by the patients themselves. Before the application of the galvanism had been continued as long as the previous process, they all said they were relieved. I relate the particulars of the two following experiments, because, independently of the principal object in view in making them, they point out two circumstances of importance in judging of the *modus operandi* of galvanism in asthmatic cases.

"The first was made on an unusually intelligent lady, of about thirty-five years of age, who had for many years laboured under habitual asthma, than whom I have known none more capable of giving a distinct account of their feelings. Her breathing was very much oppressed at the time that she first used galvanism. The immediate effect was, that she breathed with ease. She said

said she had not breathed so well for many years. Part of the relief she obtained proved permanent, and when she was galvanised once a day for about ten minutes, she suffered little dyspnœa at any time. After she had been galvanised for eight or ten days, I deceived her in the manner just mentioned. The deception was complete. She told me to increase or lessen the force of the galvanism, as she was accustomed to do, according to the sensation it produced. I obeyed her directions by increasing or lessening the force with which I scratched the neck with the wire. After I had done this for five minutes, she said the galvanism did not relieve her as usual, and that she felt the state of her breathing the same as when the operation was begun. I then allowed the galvanism to pass through the chest, but only through the upper part of it, the wire in front being applied about the middle of the sternum. She soon said that she felt a little relief; but although it was continued in this way for ten minutes, the relief was imperfect. I then directed her to apply the wire in front to the pit of the stomach, so that the galvanism passed through the whole extent of the chest, and in a minute and a half she said her breathing was easy, and that she now experienced the whole of the effect of the former applications of the remedy.

"To try how far the effect of galvanism in asthma arises merely from its stimulating the spinal marrow, in a young woman who had been several times galvanised in the usual way, the wires were applied to the nape of the neck and small of the back, and thus the galvanic influence was sent along the spine for nearly a quarter of an hour. She said her breathing was easier, but not so much so as on the former applications of the galvanism; and on attempting to walk up stairs she began to pant, and found her breathing, when she had gone about half way, as difficult as before the galvanism was applied. She was then galvanised in the usual way for five minutes: she now said her breathing was quite easy, and she walked up the whole of the stairs without bringing on any degree of panting, or feeling any dyspnœa. The above experiment was made in the presence of four medical gentlemen. This patient, after remaining free from her disease about half a year, returned to the Infirmary, labouring under a slighter degree of it, and experienced immediate relief from galvanism. The disease seemed to have been renewed by cold, which had at the same time produced other complaints. This is one of the cases above alluded to in speaking of the permanency of the good effects of galvanism. On the return of this patient to the Infirmary, two or three applications of galvanism, combined with means which had given no permanent relief to the dyspnœa previous to her first using galvanism, now

soon

soon removed it. When she first used galvanism, it required its constant employment once or twice a-day for several weeks to produce the same effect. There is reason to believe she will remain well if she can avoid taking severe colds.

"Many medical gentlemen have frequently witnessed the relief afforded by galvanism in habitual asthma, and Mr. Cole, the house-surgeon of the Worcester Infirmary, authorises me to say, that no other means there employed have been equally efficacious in relieving this disease.

"Observations similar to the foregoing, there is reason to believe, will be found to apply to dyspepsia; but as I have made but few trials of galvanism in this disease, except where it was complicated with asthma, the removal of which no doubt contributed to a more healthy action of the digestive organs, I cannot yet speak with certainty of its effects in this disease. In some, galvanism, at the time of its application, occasions a tendency to sighing; and in some, in whom it removed the dyspnœa, it seemed to occasion a sense of sinking referred to the pit of the stomach. This occurred in several instances, and was relieved by small doses of carbonate of iron and bitters.

"That I may convey to the reader as correct an idea as I can of the effects of galvanism in habitual asthma, I shall concisely relate the particulars of a few of the most, and of the least, successful cases, in which it was employed.

"Richard Morgan, a blacksmith, æt. 50, had laboured under severe habitual asthma for seven months, during which he had been better and worse for a few weeks, but never free from dyspnœa. He was much troubled with a cough, the expectorated matter being thick, and of a yellowish colour. The dyspnœa was particularly severe at the time he was galvanised, and had been so for about a fortnight. The first application of the galvanism relieved him. He was galvanised only for three days, about ten minutes each day, before he declared himself to be perfectly well. He returned to his work, which he had been obliged to abandon, after the second application of the galvanism. After its third application he performed as hard work, and with as much ease, as he had ever done.

"He remained free from dyspnœa till it was renewed, several weeks afterwards, by his getting drunk. Galvanism relieved him as readily and effectually as at first. It is now ten months since he first used this remedy, during which he has had several returns of dyspnœa, but it has never been so severe as before he was galvanised; and when it has been such as to induce him to have recourse to galvanism, he has always experienced from it immediate relief. He ascribes the returns of his disease to his being exposed to severe and sudden heats and chills.

"Mary

"Mary M'Konchy, æt. 28, a gloveress, had been afflicted with habitual asthma for four years, and under my care about one year, during which she had tried all the usual means with very imperfect relief, she had some languor in the biliary system, but little inflammatory tendency. The breathing was, in a few minutes, rendered easy by galvanism, and after the second application of it, it remained so. She now experienced no inconvenience from exercise, which had not at any time been the case for four years.

"In about three weeks after she had been galvanised she experienced some return of the dyspnœa. It was wholly removed by a blister, which had often been tried, previous to her being galvanised, with but little and very temporary relief. She complained of a sense of sinking at the stomach for some time after the use of the galvanism, which was removed by carbonate of iron and bitters. This effect of galvanism seemed often to be most felt when it gave most relief to the dyspnœa, seeming to come in place of the latter. I have hitherto found it easily removed by the above means. It is now many months since this patient was galvanised, and she remains well.

"Hannah Cooke, æt. 20, a servant, had laboured under habitual asthma for two months, and tried various medicines without relief. She was in a few minutes relieved by galvanism, and after three applications of it remained quite well. It is now five or six weeks since she was galvanised.

"I could mention several other cases, in which I witnessed the same sudden and permanent relief from galvanism, as in those here related.

"Isaac Radley, æt. 68, a labourer, formerly a soldier, had been ill fourteen years. His asthma was caused by sleeping in camp in Holland. He had never been able, during the above time, to walk at the usual pace without bringing on the dyspnœa, although he had sometimes been pretty free from it when he was still; at other times he had been constantly oppressed with it, and obliged wholly to abandon his work. At the time he used the galvanism, he was affected with the most severe dyspnœa, which only allowed him to move, and that with difficulty, at the slowest pace; he had been in this state for half a year. This was the longest and most severe fit he had ever had. He was relieved in a few minutes by the application of galvanism. He could perceive its beneficial effects for twenty-four hours after its application. It was used daily with the same immediate relief. Its permanent good effects gradually increased, and after he had been galvanised for about ten minutes each day, for between two and three weeks, his breathing remained quite easy. He could now not only walk, but, as I several times witnessed,

run

run without any dyspnœa. He complained of the sense of sinking at the pit of the stomach after the dyspnœa had left him, which, as in the case just mentioned, was readily removed by the carbonate of iron and bitters. He now said his digestion was much better than it had been previously to the use of the galvanism. Those whose breathing had been much relieved by galvanism, often made this observation, although they had not experienced the sense of sinking, and consequently had used no stomachic medicines.

"I saw this man, several months after he had ceased to use galvanism, working as a brick-layer's labourer. He said he had no feeling of dyspnœa, and had been quite free from it since he had used the galvanism.

"In general, where galvanism gave such complete and permanent relief, as in Radley's case, its effects were more speedy, some degree of dyspnœa for the most part remaining in protracted cases.

"The following are the most unsuccessful cases, which either Mr. Cole or I could recollect.

"Martha Davies, a servant, æt. 40, had laboured under habitual asthma for five years. She was relieved on the first application of galvanism, and said her breathing was quite easy; but she was not always equally relieved by it, sometimes it gave comparatively little relief. The more permanent relief afforded, was also different at different times, never complete. She was galvanised for about three weeks, but not daily, her business preventing her regular attendance; she used the remedy in all about thirteen or fourteen times. It was impossible to prevent her drinking a great deal too much malt liquor.

"It is now about half a year since she was galvanised, during which she says both her breathing and digestion have been better than for the preceding five years. She thinks the digestion as much improved as the breathing. She has had no very bad attacks of dyspnœa, and has been much less subject to bilious attacks. She is now occasionally so well that she can run without inconvenience, which she could never do during the above time, but, in general, her breathing, though in a less degree than formerly, is still oppressed.

"Mary Clark, a servant, æt. 24, had laboured under habitual asthma for about a year. The dyspnœa was always quickly relieved by the galvanism, although she seemed to experience little, if any, permanent relief from it. She had more pain in the stomach than is usual in such cases, and the galvanism seemed to increase it. She was cured by an alterative course of medicines and evacuations from the region of the stomach, and did not use galvanism for the last fortnight. She had used it at first

first daily for a fortnight, and twice afterwards for a week each time.

"As far as I can judge, from having observed the course of many cases of this kind, her recovery would neither have been so speedy nor complete if she had not used galvanism.

"Rachel Hooper, æt. 29, a servant, had laboured under severe habitual asthma for about a year, with considerable inflammatory tendency. Her breathing was relieved in a few minutes by galvanism, but not completely. For about eight or ten days, during which she was galvanised daily for about ten minutes, she derived from it considerable relief, both immediate and permanent. It then began to fail to give relief, and in a few days gave none. The epigastric region was now very tender on pressure. This symptom was relieved in the space of a few days by local blood-letting, blistering, and small does of calomel. The relief afforded by the galvanism was now greater than at first, which seemed to arise from the disease not being so severe as on the first use of the remedy, for some part of the good effects of the galvanism had remained. After this she was always relieved by it as long as she continued to use it, which was for several weeks. The permanent relief she experienced from it was also great, although she still at times laboured under a considerable degree of dyspnœa. About half a year ago, she left Worcester with a promise to return, if she should get worse. I have heard nothing of her since.

"She said nothing else had given her so much, either immediate or permanent relief, as the galvanism had done. She had been for several months in the Infirmary under other plans of treatment before she used the galvanism. All the patients whose cases I have mentioned were galvanised at the Infirmary.

"The following is a remarkable instance of permanent, though imperfect relief from galvanism, in the disease before us. A woman who had for many years laboured under severe habitual asthma was incautiously galvanised with such a power as occasioned severe pain. No entreaty could induce her to submit to a repetition of the galvanism, although it had immediately relieved her breathing. The dyspnœa soon recurred, but she told me many months afterwards that it had never been so severe since she was galvanised, and that she had ever since been able to carry water in buckets from the river, which the state of her breathing had not for a long time previously allowed her to do.

"If the reader will compare these cases with the general observations which I have had occasion to make on the effects of galvanism in habitual asthma, he will be enabled to form a pretty correct estimate of what he may expect from it in this disease.

"When we compare them with the experiments laid before the reader in the preceding Inquiry, the question naturally arises,

 Whence

Whence proceeds the permanent relief obtained in them? The galvanic experiments lead us to expect relief to the dyspnœa while the stream of galvanism passes through the lungs; but on what principle shall we explain the permanency of the relief afforded? The following observations appear to throw some light on this subject. There are two ways in which an organ may be deprived of its nervous influence, either by a failure of due action in the brain and spinal marrow, the sources of nervous influence, or a failure of due action in the nerves of the organ affected by which this influence is conveyed. It is no longer conveyed by a nerve which has been divided, or around which a ligature has been thrown. Now we have reason to believe that habitual asthma arises not so much from a fault in the brain and spinal marrow, as in the nerves of the lungs; because, did the degree of dyspnœa, which we often witness in this disease, arise from failure in the general source of nervous influence, this failure must be sufficient to appear in the derangement of all the nervous functions; whereas in habitual asthma, we often find the function of the lungs alone affected; and when general failure of nervous influence is observed, it is evidently the effect of impeded respiration, appearing only after the latter has continued for some time, and varying as it varies. The effect produced by galvanism, when it performs a cure in habitual asthma, therefore, does not appear to be its having occasioned a permanent supply of nervous influence, but its having cleared, if I may use the expression, the passage of this influence to the lungs. It is not difficult to conceive that such an obstruction may exist in the nerves as cannot be overcome by the usual supply of nervous influence, though it may yield to a greatly increased supply of it; and that it may in some cases continually recur in an equal or diminished degree, while in others, being once removed, the tendency to it may cease*.

"The foregoing observations seem to explain why other means which give a temporary vigour to the nervous system, often, for the time, relieve habitual asthma; and sometimes, though rarely, cure this disease. The relief obtained from such means being in general so much less than that obtained from galvanism, I would ascribe to the former occasioning but little additional supply of nervous influence, while by the latter we can make the additional supply as great as we please."

* "What is here said is well illustrated by the effects of galvanism in apoplexy. We know that in this disease the dyspnœa arises from a failure in the source of nervous influence, and the relief obtained from galvanism corresponds with the views afforded by the experiments which have been laid before the reader. While the galvanism passed through the lungs the dyspnœa was as much relieved as in habitual asthma, but when it ceased to pass through them, the relief lasted no longer than was necessary for the reaccumulation of the phlegm."

Early

Early in November will be published, by Thomas Jones, optician, No. 62, Charing Cross, The late Mr. Ferguson's Astronomical Planisphere, showing the day of the month, change and age of the moon, the places of the sun and moon, and stars of the first, second and third magnitude. Likewise his Astronomical Rotula, showing the change and age of the moon, the motion of the sun, moon, and nodes, with all the solar and lunar eclipses from the year 1817 to 1864, with descriptions of their uses.—The calculations continued by the Rev. Mr. L. Evans, of the Roy. Mil. Acad.

The price of the Planisphere, consisting of three plates with a circular motion, on pasteboard will be eight shillings; and in boards about thirteen inches square, as a book ten shillings. The Rotula consisting of five plates, eight shillings plain, and ten shillings in boards. The same size as the Planisphere.

Mr. Thomas Forster has just published "Observations on the Phænomena and Treatment of Insanity, &c." being supplementary to his Observations on periodical Diseases and on the Periods of Insanity.

The manuscripts of the late Mr. Spence of Greenock were some time ago submitted to Mr. Herschel, who has selected the most complete for publication. It will gratify the students of pure mathematics to understand, that the volume now preparing, and which will be published in the course of the spring by Messrs. T. and G. Underwood, contains, besides the ingenious Essay on Logarithmic Transcendents, unpublished tracts in the same class of the science, equally new and elegant. A biographical sketch of the author by his friend Mr. Galt will be prefixed to the volume.

LI. *Intelligence and Miscellaneous Articles.*

TRIUMPH OF SCIENCE.

THE subjoined decided and honourable testimony given to the originality and utility of Sir Humphry Davy's discovery of the safety-lamp for miners, deserves to have a more durable record than the ephemeral columns of a newspaper, and our readers, we are sure, will therefore thank us for giving it a place in our pages.

The coal-owners of the rivers Tyne and Wear, the body most extensively benefited by Sir Humphry Davy's safety-lamps for preventing explosions in coal-mines, have shown their sense of the importance of the discovery to their interests, and those of humanity, by presenting Sir Humphry with a very handsome service of plate, of the value of nearly two thousand pounds. The ceremony of the presentation of it took place on Saturday, October the 11th, when a grand dinner was given to Sir Humphry by

by the Coal Proprietors and Owners at the Queen's Head at Newcastle, where the plate was exposed for public inspection, and the designs, taste, and execution, equally admired. J. G. Lambton, esq. M.P. for the county of Durham, was in the chair. There were present: The Mayor, Sheriff, and Town Clerk of Newcastle; the Rev. Dr. Gray; J. Collinson, and J. Hodgson; Messrs. Warren, Lamb, Baker, Lorraine, Buddle, Ellison, Botts, Brown, Mowbray, Robinson, and about fifty other gentlemen.

After the King and the Prince Regent, the Queen and Royal Family, had been drank, Mr. Lambton rose, and presented the service of plate to Sir Humphry Davy, and addressed him nearly in these terms, with great strength of feeling:

"Sir Humphry—It is now my duty to fulfil the object of this meeting, in presenting to you this service of plate, from the coal-owners of the Tyne and Wear, as a testimony of their gratitude for the services you have rendered to them and to humanity. Your brilliant genius, which has been so long employed in extending, in an unparalleled manner, the boundaries of chemical knowledge, never accomplished a better object, nor obtained a nobler triumph. You had to contend with an element of destruction, which seemed uncontrollable by human power, which not only rendered the property of the coal-owner insecure, but kept him in perpetual alarm with respect to the safety of the miner, and often exhibited to him scenes of death and heart-rending misery. You have increased the value of an important branch of productive industry; and, what is of infinitely greater importance, you have contributed to the preservation of the lives and persons of multitudes of your fellow-creatures. It is now nearly two years that your safety-lamp has been used by hundreds of miners, in the most dangerous situations, and under the most trying circumstances. Not a single failure has occurred; its absolute security is demonstrated. I have, indeed, deeply to lament more than one catastrophe produced by fool-hardiness and ignorance in neglecting to use it, but even these dreadful accidents, if possible, exalt its importance. If your fame had needed any thing to make it immortal, this discovery alone would have carried it down to future ages, connected with benefits and blessings. Receive, Sir Humphry, this permanent memorial of our profound respect and high admiration—a testimony, we trust, equally honourable to you and to us. We hope you will have as much pleasure in receiving, as we have in offering it; long may you live to use it; long may you live to pursue your splendid career of scientific discovery, and to give new claims to the gratitude and praise of the world!"

Sir Humphry Davy having received the plate, spoke nearly in the following words:

"Gentlemen—I find it impossible to reply in an appropriate manner

manner to the very eloquent and flattering address of your distinguished chairman. Eloquence, or even accuracy of language, is incompatible with strong feeling, and, on an occasion like the present, you will give me credit for no small degree of emotion.

"I have been informed, that my labours have been useful to an important branch of human industry connected with our arts, our manufactures, commerce, and national wealth. To learn this from such practical authority, is a high gratification to a person whose ardent desire has always been to apply science to purposes of utility. It has also been stated, that the invention, which you are this day so highly honouring, has been subservient to the preservation of the lives and persons of a most useful and laborious class of men: this coming from your own knowledge, founded upon such ample experience, affords me a pleasure still more exalted—for the highest ambition of my life has been to deserve the name of a friend to humanity. To crown all, you have as it were embodied these sentiments in a permanent and magnificent memorial of your good opinion. I can make only imperfect and inadequate efforts to thank you. Under all circumstances of my future life, the recollection of this day will warm my heart; and this noble expression of your kindness wil' awaken my gratitude to the last moment of my existence."

Mr. Lambton's speech, and Sir Humphry's reply, were received with loud acclamations; as was likewise Sir Humphry Davy's health, which Mr. Lambton gave with three times three, and introduced in another eloquent speech, still further extolling the merits of the lamp, and the disinterested manner in which it had been presented to the public.

Sir H. Davy, in reply, said, that he was overpowered by gratitude, by these reiterated proofs of their approbation—that his merits were far overrated—that his success in their cause was owing to his following the path of experiment, discovered by philosophers who had preceded him—that he would willingly divide their plaudits with other men of science, and claim much for the general glory of scientific discovery in a long course of ages. He referred to the great increase of wealth and power to the country, within the last fifty years, by scientific inventions, which could not have existed without coal-mines; the improvement of the potteries, the steam-engine, and the discovery of gas lights. In referring to the steam-engine, he said, "What an immense impulse has this machine given to arts and manufactures! how much has it diminished labour, and increased the real strength of the country, far beyond a mere increase of population! By giving facilities to a number of other inventions, it had even a moral effect in rendering capital necessary for the perfection of labour, credit essential to capital, and ingenuity and mental energy a secure and dignified species of property.

Science was of infinitely more importance than could at first view be supposed to the state, for no source of wealth or power was entirely independent of it: and no class of men were so well able to appreciate its advantages as the gentlemen whom he had the honour of addressing; for they not only derived from it the means of raising their subterraneous wealth, but likewise those of rendering it useful to the public. In various manner it was science that had made pit-coal such an instrument in the hands of the chemist and mechanic, so as to make the elements fire and water perform operations which formerly demanded human labour; and to convert the productions of the earth into a thousand new forms of beauty and use. Sir H. Davy said, that it was in pursuing those methods of analogy and experiment by which the mystery had become a science, that he had discovered the safety-lamp—that he had registered the whole progress of his researches in the Transactions of the Royal Society, in papers which that illustrious body had honoured by their biennial medal, and that in those papers he had acknowledged the slightest hints, or offers of assistance, that he had received.—He stated this, not from vain glory, but on account of certain calumnious insinuations which had arisen, not in the scientific world, for to that the whole progress of his scientific researches was well known, but in a colliery. He must ever treat these insinuations with contempt; and, after the indignation which had been expressed against them by the coal-owners in general, he could not have any anxiety on the subject, nor should he have referred to it at all, but that he had every reason to believe that the persons amongst whom these insinuations originated were extensively benefited by, and were constantly using, his invention. And that it was far from his expectation that such persons would have employed their respectable connexions in mean attempts to impeach the originality of a discovery which was given to them in a disinterested manner, and for which no return was required, but an honest acknowledgement of the benefit, founded upon truth and justice. "I (said Sir H. D.) do not envy them their feelings, particularly at the present moment. I do not wish to inquire into their motives; I hope, however, that their conduct has been prompted by ignorance rather than malevolence, by misapprehension rather than ingratitude. It was a new circumstance to me, that attempts to preserve human life, and prevent human misery, should create hostile feelings in persons who professed to have similar objects in view. I have had some opposition, much labour, and more anxiety during the course of these researches; but had the opposition, the labour, and the anxiety been a thousand times as great, the events of this day would have been more than a compensation."—(*Great plaudits*). Sir Humphry, in drinking the health of the company, offered as a sentiment—"Prosperity to the Coal Trade."

The

The Chairman proposed the health of the Duke of Northumberland, the Lord Lieutenant of the county.

The Manager of his Grace's coal concerns returned thanks, and read an extract of a letter from the Duke, expressing his admiration of the object of the meeting, and his conviction of the great benefit that had resulted to science, and humanity in general, and the coal trade in particular, from Sir H. Davy's discoveries.

Mr. Lambton gave the health of the Mayor of Newcastle, who returned thanks, and gave the health of the Chairman, which was drank with three times three, and great plaudits.

Mr. Lambton, after returning thanks, again alluding to the object of the meeting, stated his own desire upon all occasions to promote, to his best endeavours, the interest of the coal-trade.

The health of the Bishop of Durham was drank. The Rev. Mr. Collinson, his representative on this occason, said that nothing but the age and infirmity of the venerable Bishop prevented him from being present; that no one was more deeply interested in the object of their meeting. Mr. Collinson said, that whatever gratification Sir H. Davy received from the enthusiasm with which his invention was received by men so well able to appreciate it, yet that it must be infinitely more gratifying to him to know, that men now living, and their remotest posterity, would be indebted to him for their safety; and that he was an instrument in the hands of Providence, not only for protecting human life, but for preserving human happiness.

Mr. Lambton gave "Mr. Buddle, and the Viewers of Newcastle," stating, that the coal-owners owed much to the courage and sagacity with which they investigated danger, and the skill which they used in avoiding it; and paid many just compliments to their science, as well as to their humanity.

Mr. Buddle, in returning thanks, said, that Sir Humphry Davy's lamp offered them resources in the art of mining which they had never hoped for, enabled them to work coals which could never even have been explored, and above all, took from their minds a heavy weight of responsibility.

Sir Humphry Davy gave the health of the Rev. Dr. Gray, a gentleman, he said, by whose enlightened philanthropy his attention had been first turned to the subject.—Dr. Gray returned thanks.

The following toasts were given by the Chairman:

"The Union of Science with Humanity."—"The Trade of Tyne and Wear."—"The Members of Newcastle."—"The Rev. John Hodgson."

Sir H. Davy said he had spoken of the general benefits resulting from science; he was sure they would drink with pleasure

 the

the health of a most venerable and distinguished friend to science—Sir Joseph Banks, the President of the Royal Society, who in youth had endeavoured to extend the limits of human knowledge, amongst difficulties and dangers; who, in his advanced age, was the patron of every useful object; and who, through his whole life, had devoted his fortune and his time to the purposes of science."

Mr. Robinson gave "The Members for the County of Durham."—Mr. Lambton returned thanks.

At ten o'clock Mr. Lambton and Sir H. Davy took their leave amidst the enthusiastic applauses of the meeting, when Mr. Wm. Lamb took the chair, and harmony and conviviality were kept up till a late hour. Never was there a more agreeable meeting, and as the object of it was one of convivial benevolence, so the effect of it was universal hilarity.

STEAM ENGINES IN CORNWALL.

From the Monthly Report for September, it appears that during that month the following was the work performed by the engines reported, with each bushel of coals.

		Water lifted 1 foot high with each bushel.	Load per square inch in cylinder.
23 common engines averaged		23,099,400	various.
Woolf's at Wheal Vor	..	38,894,222	15·5 lib.
Ditto Wh. Abraham*	..	40,310,194	16·8
Ditto ditto ..	..	26,138,822	[illegible]·3
Ditto Wh. Unity†	..	29,557,019	13·1
Dalcouth engine ..	..	43,031,945	11[illegible]
Wheal Abraham ditto	..	35,128,397	10·8
United Mines ditto ..	..	30,716,538	18·1
Wheal Chance ditto	..	38,832,427	13·1

VOLTAIC ACTION, SAFETY-FURNACE, ETC.

To Mr. Tilloch.

Sir,—Having ascertained the influence of atmospheric air in increasing the intensity of the Voltaic action, I was next desirous, in following up the views of M. Dessaignes, to ascertain the effect *of an exalted temperature*. For this purpose the plates were heated highly in a sand-bath and plunged into the acid medium in the porcelain cells. By this means I ignited with the three porcelain troughs adverted to, still preserving the same diluted acid, *eleven inches and a half* of platinum wire. The experiments with charcoal and metallic laminæ were proportionally brilliant. I next raised the temperature of the acid solution up to 130° F. and obtained results nearly as striking.

* Has had considerable lets this month to repair boilers.

† Working part of this month without the aid of the small cylinder.

We

We have thus provided another mean for obtaining an increased action.

In referring to the apparatus I have proposed for drawing off and consuming the explosive hydrocarbonate of the mine, I should have mentioned, that when one, two or more pipes are used, the orifices of the others must be shut by means of proper appendages. By allowing the urn to *rest in water*, it will always be kept *cool*;—or a current of water being permitted to enter from below, through a small aperture; we should have, besides the safety afforded by the wire-gauge, that of an atmosphere of *steam*;—and if the position of Sir H. Davy be founded in truth, that *flame* is an exhibition of temperature above a *white heat*, and that the wire-gauze serves merely to *cool down* the flame below that increment which is the grade of incipient flame,—a *fillet of wire-gauze* in the interior of the urn, thus, would present a number of cooling surfaces, by incepting the included flame, and such a convolute, or spiral partition of wire-gauze, would yield a security as *ample* and as *absolute* as the safety-lamp*.

I exposed to the action of the compound gases in the oxy-hydrogen blowpipe, a fragment of a *meteoric stone* which fell at *Pulrose* (one mile from Douglas, Isle of Man), about twenty years ago, during a thunder-storm. It tore up the ground with considerable violence, killing a mare and foal at the same time. This meteorolite appears somewhat like a dark pumice-stone,—of low specific gravity,—and containing a few small white specks resembling *leucite* when exposed to high increments of temperature.

It exhibited before the blow-pipe: first, an intense *most vivid light*,—then entered into *fusion*, and passed into a *black glass*.

I also introduced before the ignited gaseous mixture a piece of what has been long known here under the name of polishing powder. This portion of it which I found *in situ* resembled *asbestos*, having a ligniform structure, but crumbling into a soft powder between the fingers. I found it in contact with decomposing granite and quartz interspersed with needle schorl; indeed, I have specimens which I found in the same place, composed of masses of needle and compact schorl exhibiting the *various transitions* into this substance.

Before the blow-pipe it was characterized by a vivid light like

* I apprehend that the convolute would present but *one* cooling surface, namely *the exterior*; the inner convolutions would be in the situation of a piece of wire-gauze within a safety-lamp.—Edit.

magnesia

magnesia when introduced before the condensed ignited gas, entered into rapid *fusion*, and formed a beautiful *black bead.*

I am, sir, your obedient humble servant,

Douglas, Isle of Man, Sept. 17, 1817. J. MURRAY.

The following errata appear in my late paper: Page 143, line 3, read "*while*" instead of *until*. Page 144, read "*of the* corrosive salt." Page 146, read Mr. Porrett jun. J. M.

PROCESS FOR PREPARING ACETATE OF POTASH:—COLOURING MATTER OF DRAGONS BLOOD EXTRACTED BY QUICKLIME.

To Mr. Tilloch.

SIR,—I beg leave to offer to the consideration of your chemical readers a more convenient process for preparing acetate of potassa than that which is at present followed, in the way now practised; viz. that of saturating subcarbonate of potassa with distilled vinegar. It almost invariably happens that the solution is of a *brown* colour, probably arising in part from the presence of some extractive matter, and partly from the partial combustion of the acid during the ebullition. In order to remove this impurity the solution is evaporated to dryness, and the residuum is melted by a gentle heat and left to crystallize. If the solution of the acetate be made in the way which I shall proceed to direct, it is *colourless*, and consequently does not require the evaporation to dryness and subsequent fusion.

Let 120 parts of subcarbonate of potassa and 300 parts of superacetate of lead be separately dissolved in as little water as possible; the solutions are to be mixed together, the carbonate of lead will be precipitated, and the acetate of potassa will remain in solution, which may be evaporated until it becomes somewhat thick, and then set aside to crystallize.

I am unacquainted with the quantity of acetate yielded by the above proportions; as while the solution was evaporating, an accident happened to my apparatus.

I obtained from the 300 grains of *superacetate* only 160 grains of *carbonate* of lead; from which circumstance I am induced to question the accuracy of Thenard's statement of the proportions of the component parts of the former salt; viz. oxide of lead 58; acetic acid 26; water 16: for, supposing his account to be correct, I did not obtain the full proportion of oxide, without reckoning the carbonic acid: the whole of which, as no effervescence occurred on mixing the solutions, must have entered into combination. I am, sir, yours respectfully,

July 27, 1817. LITHOPHILUS.

P. S. I have recently found that the colouring matter of the resin vulgarly denominated dragons blood, may be extracted by quicklime, almost if not quite as well as by caustic alkali.

DEATH BY LIGHTNING.—QUERIES.

To Mr. Tilloch.

SIR,—During a visit to a friend in Herefordshire, I was desired to examine the body of a man whose life had been suddenly destroyed by lightning at Colwall near Ledbury. The wife of the deceased obstinately refusing permission to open the body, my examination was confined to the effects of the electric fluid on the external surface. On viewing the head, I found the hair and the beard of the left side singed, and the skin of the ear, cheek, and upper part of the neck perfectly black, but entire. Between the shoulders there was another black spot of the size of a crown, exactly over the spine, and on the outside of the thigh of the right side just above the knee, there was another black spot which I could scarcely cover with my hand. The parts of the shirt and flannel lining of the breeches and jacket which covered the injured skin were charred, but neither the exterior parts of the small-clothes (which were made of corduroy) nor of the jacket were burnt, the electric fluid having only occasioned a laceration resembling an incision made by a sharp instrument. It appears that the electric matter entered the left side of the head, passed through the chest and abdomen in an oblique direction, and escaped just above the knee on the opposite side of the body. Whether the fluid entered or escaped at the spot on the back, I am at a loss to say; but from the external part of the jacket not being burnt, I suspect a quantity escaped there. The spots were perfectly black, and exhibited the same appearance as is produced by the caustic alkali after remaining several hours on the skin; and from its flabby state, the mischief was no doubt deep. Whether the fluid produced the same effect on the internal parts through which the fluid passed as it did on the skin, is a question which I shall be obliged to you, or some of your readers who have ascertained the fact in a similar case, to answer. The man at the time the accident happened was under an oak-tree, and when the lightning struck him he sprung forward and fell on his face; soon after which there was a second flash, and this might have produced the spot between the shoulders; but if so, where did it escape?

About twenty years ago I had an opportunity to examine a man who was struck dead by lightning in a field near Hereford, with an umbrella over his head. On that man the electric fluid produced no evident effect either externally or internally. The brain had a sulphureous smell.

Queries. Did the passing of the fluid through the umbrella prevent its burning the body? As no apparent injury was done to the body, how are we to account for its effects in destroying life?

life? Did it terminate life by destroying the electrical powers of the brain? I hope some of your readers will, through the medium of your valuable work, favour me with some remarks on these cases, and replies to my queries. I am, sir, your constant Reader, R. R.

Piccadilly, Oct. 17, 1817.

NEW SCALE FOR THE MOUNTAIN BAROMETER.

Professor Bertoncelli of Verona has contrived an ingenious method of adapting a graduated measure to the common scale of the barometer, to indicate the height of mountains without the necessity of calculating for the different degrees of temperature, &c. To the common scale he adapts a corresponding one, dividing the inches into 100, placing his zero at mean pressure, and ascending both ways in numeration from that point. This scale is surmounted by a brass revolving cylinder on which are graved four different series of lines; one perpendicular divided like the preceding; another of ten diverging lines which ascend the whole length of the cylinder, and the rationale of which the Professor has not stated;—these lines are again partially intersected by two series of four lines diverging at right angles from the point of zero, and designed to indicate the correction for difference of temperature. The whole cylinder revolves by means of a screw, and acts in conjunction with the counter scale of the barometer; it is accompanied by a vernier, which is commanded by two or three screws to the point of correction; while this vernier is also to act in correspondence with a common nonius placed on the inch scale opposed to the surface of the mercury. This complex machine Professor Bertoncelli calls an Ipsographic scale, which nevertheless has still to be read off and calculated by the aid of logarithms. If he could find a metal which would not contract with cold, then his series of screws and tangent lines might be useful; and if logarithms were more familiar than common addition or subtraction, this instrument might prove of much general utility.

OPTICS.

A very interesting case has just occurred, of a person born blind being restored to sight by the means of a surgical operation:—A native of Burdwan, of the age of eighteen, was lately sent by his family to Dr. Luxmore, of whose success in the removal of the cataract they had heard by public report. The operation was performed on the 26th, and in six days he began to see and distinguish objects. After the celebrated case of Dr. Cheselden's patient, whose sensations have been so minutely and philosophically laid before the public, it can hardly be expected that any discovery regarding the origin of our ideas of figure, distance, or quantity, could be extracted from the observation of an ignorant

norant country boy, who, unaccustomed to think abstractedly, is little able to describe the gradual improvement of his intellect, under this sudden and astonishing introduction to the visible world. He confirmed, however, with readiness the conclusion, so obvious from the feelings of Dr. Cheselden's patient, that our common judgement of figure, quantity, and distance, is not an inherent faculty in the mind, but a practical result, from the ever-repeated experiment of comparing the perspective with the actual figure, bulk, or distance. For a cricket-ball was put in one hand, and a cube of soap in the other, and he was desired to describe their shape; he was unable to do it by his newly acquired and inexperienced vision, and was obliged to have constant recourse to the more practised sense of feeling. When any object is presented to him, although he can without hesitation declare its colour, he is wholly unable to decide on its quality, until he is allowed to handle it.—*Bengal Paper.*

HYBERNATION OF SWALLOWS.

Extract of a Letter from Joseph Wood, *esq. to a Gentleman in Washington.*

"Marietta, June 30.

"I came to this country in the autumn of 1785, and resided at Belleville about three miles below this place, on the Virginia side, till 1791. During my residence there, I observed one evening a little after sunset, a vast number of swallows collected together high in the air, and hovering over a particular spot; this was in autumn, when the weather began to grow cool. Having been informed by some of my school-mates, when a boy, that they had seen swallows dive into a mill-pond, and disappear, I was determined to watch these, and in about ten or fifteen minutes, as darkness approached, they lowered their flight, and concentrated in a smaller circle, and at length, to my surprise, poured into a very large hollow sycamore-tree, about seventy feet above the ground. I observed that they came out for several successive days, and returned in the evening in the same manner. In the following year, some of the settlers cut down the tree; the hollow was about six feet in diameter, and was filled six inches deep with bones and feathers, and other remains of dead birds;—such, probably, as were too old and feeble to fly out in the spring. They must have occupied the tree for many years. I have since seen two other trees that have fallen, with similar appearances."

LIST OF PATENTS FOR NEW INVENTIONS.

To Edmund Richard Ball, of Albury Mills, in the parish of Albury, Surry, for his new method of manufacturing paper of

superior

superior strength and durability for bills, or notes, or other uses requiring strength.—9th August 1817.—2 months to enroll.

To Edward Biggs, of Birmingham, for his improvements in the methods of making or manufacturing pans and stails of various kinds.—12th August.—2 months.

To James Bounsall, of Crown-street, Old-street Road, Shoreditch, Middlesex, for certain improvements in the machinery used for tarring, reeling, and twisting of yarn, and forming the lissims or strands of cables and other cordage, and manufacturing rope of every size.—12th August.—6 months.

To William Geldart and John Servant, both of Leeds, Yorkshire, for certain improvements in mangles.—12th August—2 mo.

To Jephtha Avery Wilkinson, late of New-York in the United States of America, but now of Covent Garden, Middlesex, for certain improvements in the application of machinery for the purpose of manufacturing of weavers' reeds by water or other power.—23d August.—6 months.

To George Medhurst, of Denmark-street, St. Giles, Middlesex, engineer, for an arrangement of implements to form certain apparatus which he denominates the hydraulic balance, applicable to mechanical and hydraulic purposes.—26th August.—6 months.

To Mr. Tilloch.

Sir,—I take the opportunity of inquiring, through the means of your widely-extended Magazine, whether any of your astronomical correspondents observed the remarkable conjunction of *Venus* with *Regulus* on the 29th of September last. I find by M. Bode's Ephemeris, that the two stars would, at Berlin, appear within *twelve seconds* of each other: but no notice is taken of this singular phænomenon either in the Nautical Almanack, or in the *Connaissance des Tems.*

I am, sir, your obedient servant,

October 27, 1817. Astronomicus.

ASTRONOMICAL PHÆNOMENA, NOVEMBER 1817.

D. H. M.		D. H. M.	
2.23.53	☽ η ♌	12. 8. 6	☽ τ ♐
4.21.47	☽ 3ν ♍	14.21.41	☽ ε ♑
6. 0.16	☽ γ ♍	20. 8.20	☽ ο ♓
6.11. 4	☽ [illegible]	22. 0. 0	☽ in apogee
7.16. 5	☽ ☿	22. 2.54	☉ enters ♐
7.19.17	☽ λ ♍	23. 9. 6	☽ A ♉
8. 8. 8	☽ 2α ♎	25. 2.35	☽ ♂
9.11.43	☽ δ ♍	25. 5.33	☽ 125 ♉
9. 0. 0	☽ in perigee	27. 9. 2	☽ υ ♊
10.16. 5	☽ θ Ophiuchi	28. 0.27	☽ ψ 3 ♋
12. 0.20	☽ φ ♐	30. 6.49	☽ η ♌
12. 4. 1	☽ σ ♐		

METEORO-

METEOROLOGICAL JOURNAL KEPT AT BOSTON, LINCOLNSHIRE.

[The time of observation, unless otherwise stated, is at 1 P.M.]

1817.	Age of the Moon.	Thermometer.	Barometer.	State of the Weather and Modification of the Clouds.
	DAYS.			
Sept. 15	4	61·5	30·20	Rain
16	5	65·	30·20	Cloudy
17	6	67·5	30·15	Fair
18	7	58·	29·81	Rain
19	8	61·	30	Fair—rain A.M.
20	9	61·	30·15	Ditto
21	10	62·	30·14	Ditto
22	11	59·	30·03	Cloudy
23	12	60·	30·07	Fair
24	13	60·	30·06	Ditto
25	full	62·	29·60	Ditto—rain A.M.
26	15	60·	29·27	Fair—gale from the SW.
27	16	56·	29·44	Cloudy—ditto
28	17	54·	29·80	Ditto—wind N. by E.—sharp frost at night
29	18	54·5	30·09	Fine ditto ditto
30	19	52·	30·14	Ditto ditto ditto
Oct. 1	20	52·5	29 90	Ditto ditto ditto
2	21	46·5	30·21	Ditto ditto ditto
3	22	51·	30·22	Ditto ditto ditto
4	23	55·	30·39	Ditto
5	24	53·	30·47	Ditto—heavy rain A.M.
6	25	52·	30·45	Very fine
7	26	57·	30·36	Ditto
8	27	54·	30·2[illegible]	Fine
9	28	55·	30·17	Ditto
10	new	56·	30·07	Cloudy
11	1	48·5	30·16	Fair—cold rain A.M.
12	2	48·	30·20	Showery
13	3	50·	30·40	Very fine—rain P.M.
14	4	48·	30·32	Showery

METEORO-

METEOROLOGICAL TABLE,

By Mr. Cary, of the Strand,

For October 1817.

Days of Month.	Thermometer. 8 o'Clock, Morning.	Thermometer. Noon.	Thermometer. 11 o'Clock, Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
Sept. 27	55	56	48	29.50	0	Rain
28	48	56	50	·91	22	Fair
29	44	55	49	·89	36	Fair
30	49	55	50	·87	30	Fair
Oct. 1	47	54	45	·75	31	Fair
2	39	50	40	30·01	26	Fair
3	35	50	40	·14	27	Fair
4	44	55	46	·21	20	Fair
5	44	56	44	·23	36	Fair
6	42	54	43	·14	36	Fair
7	48	57	44	·10	40	Fair
8	45	50	47	·02	32	Fair
9	47	55	47	29·88	39	Fair
10	44	56	50	·89	36	Fair
11	45	50	42	·94	27	Fair
12	44	50	38	30·05	26	Showery
13	38	48	42	·26	25	Showery
14	44	50	42	·10	24	Cloudy
15	42	47	42	29·90	0	Showery
16	43	46	40	·96	0	Rain
17	42	47	42	·99	0	Showery
18	43	46	41	·78	0	Rain
19	44	47	45	·82	17	Cloudy
20	45	48	45	·90	0	Rain
21	46	48	46	·80	9	Cloudy
22	[illegible]	50	42	·82	10	Cloudy
23	45	48	45	·90	6	Showery
24	44	46	45	·80	0	Rain
25	[illegible]	50	45	·75	12	Cloudy
26	47	52	44	·70	22	Fair

[illegible] Barometer's height is taken at one o'clock.

LII. *On the Question "Whether Music is necessary to the Orator,—to what Extent, and how most readily attainable?"* By HENRY UPINGTON, *Esq.*

To Mr. Tilloch.

Blair's Hill, Cork, Sept. 25, 1817.

SIR,—HAVING been lately requested by a particular friend, to direct my attention towards the investigation of a certain subject which he considered both interesting and instructive, I complied with his wishes; and having proceeded a certain length, I now transmit you a copy, intending at a future period to complete the inquiry.

The purport of this investigation was—"Whether music is necessary to the orator—to what extent, and how most readily attainable?"

As there appeared to me, at my first setting out, some probable connexion between the intervals of speech, and the ancient division of the musical scale, I was determined, if possible, to analyse the tetrachord. Hence arose not only the question of minute division, but of concords, even to the perfection or imperfection of our present harmonical basis:—and with this part of the subject I thought it more desirable to begin.

Experiment 1. I prepared a common deal sounding-board about four feet in length, with an ordinary bridge, and sufficient steel wires for the subdivision of one single *fourth* into quarter tones, CF being the extremes.

Result. After getting the best-ear'd musicians around me, to tune, retune, alter, realter, by ear as well as by all the ancient data I could trace,—the only effect produced, in *their* estimation, whenever a quarter tone was struck, was that which would necessarily be produced by an instrument out of tune.

What can we infer from hence? That modern ears are no more prepared for the reception of the real diesis or quarter tone, than the ears of our earliest ancestors would have been for that of our present semi-tonical division. Here a very important question presents itself: How happens it, that at this very day (if we may believe Dr. Burney) the *Arabian* scale is more minutely divided than ours; their octave containing twenty-four quarter tones, for all of which there are particular denominations? Must it not have arisen from excessive cultivation, the ear having been previously satiated with the semi-tone? Dr. B. is right perhaps in asserting that such division is incompatible with modern harmony. But what of this? Does it prove the superiority of modern European ears, or the superiority of our system?

system? It is idle to speak of *Nature*—we are all the children of Art. But in regard to our *senses*, is there any rational ground for asserting that our ears are more infallible than our eyes? Early impressions will produce within us certain ideas of beauty which no subsequent comparison can efface: Hence the tottering foot and sugar-loaf head are held in greater estimation by the Chinese, than those of the most perfect statue ever said to have been formed by the chisel of Praxiteles. We may next proceed to

Experiment 2. being one certain, and perhaps the only method of tuning the soni stabiles or immoveable tones of the disdiapason, agreeably (as I conceive) to the laws of the ancient system; that is, with three conjunct, and one disjunct, tetrachords; for which purpose I employed a common piano-forte.

Let the disdiapason or double octave be represented by the following letters, taking C as the fundamental:

C C* D D* E F F* G G* A A* B *c c* d d* e f f* g g* a a* b* c

1st. Tune D to any desired pitch
2dly. —— G a perfect 4th from D
3dly. —— *c* a perfect 4th from G
4thly. —— C a perfect octave from *c*
5thly. —— *f* a perfect 4th from *c*
6thly. —— c a perfect octave from *c*
7thly. —— *g* a perfect 4th (downwards) from c

This tuning was effected by means of a monochord, the comparative lengths of sounding wire being

	Inches.
For the fundamental, suppose	1000
For its 4th above, $\frac{3}{4}$ths of 1000 ..	750
For its octave, $\frac{1}{2}$ of 1000	500

The ultimate soni stabiles will therefore exhibit themselves in the following order: every other note being subject to the proposed adjustment of the performer.

C D G *c* *f g* c

Now this arrangement, which insists upon no more than two intermediate notes in either octave, will be found upon trial to differ so very little, if at all, from that mode of tuning most agreeable to a cultivated ear, that we must consider it (at least for simple melody) as a mere well-regulated outline.

I could here wish to examine, why the *fourth* was considered by the ancients as the most perfect conchord; so perfect indeed as to constitute the main regulator of the scale;—but the documents of antiquity are wanting. We must therefore resort to a modern experiment, which I find recorded in one of our Cyclopædias (I believe Rees's), which exhibits the question in a singularly striking manner.

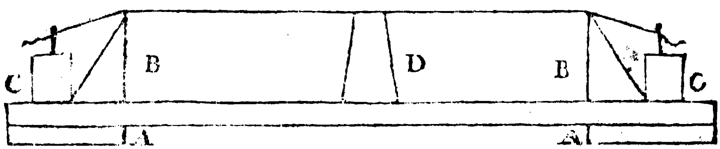
Not

Not content with the assertion of the writer, who made the experiment with a wire several yards in length, I constructed a simple monochord (that of which I have just spoken), and found the results to correspond. As this very simple instrument is not only amusing, but on many occasions instructive, I shall describe it.

Monochord.

Take an even strip of deal, free from knots, about three feet long, five inches broad, and three-quarters thick. Plane it fair, and glue upon each end a piece of hard wood about an inch and a half in height. Stretch a steel wire horizontally over these by means of two upright iron pegs suited to a common tuning-hammer. Lastly, procure a perpendicular bridge of hard wood, about an inch long, whose base may be three-quarters, and whose summit about one-quarter inch in breadth: let it move freely to and fro beneath the wire, (merely in contact and no more, lest the *pitch* should be altered,) and press *it* (the wire) down upon the aforesaid bridge, at the destined mark, with your nail, or, which is better, with a small oblong square piece of hard timber. The monochord is thus complete; and by way of a sounding-board, you have only to place it on a table, or on the leaf of a piano-forte.

A rough side-view may explain it better.



A A, Blocks which serve for feet.
B B, Permanent bridges, each one inch and a half high.
C C, Blocks in which the pegs are inserted.
D, Moveable bridge.

Let me now describe the experiment which proves the gradation of our concords.

Experiment 3. Take a strip of fine soft paper (news paper will answer) about one inch and a half long and half an inch broad, bent longitudinally in a triangular form, like a *saddle*. With one extremity of this saddle (its apex being upward) the string while sounding is to be gently pressed at given points, and tones different from the original will be strongly perceptible.

Thus,

Thus,

Points of contact.	Sounds which would be produced at such points were the bridge so situate, and the wire struck in the usual way.	Harmonic sounds resulting from gentle pressure by the soft paper.
Centre.	Octave. ..	Octave.
$\frac{3}{4}$ point.	Fourth. ..	Double Octave.
$\frac{2}{3}$ do.	Fifth. ..	Octave of the Fifth.
$\frac{5}{8}$ do.	Minor 6th ..	Triple Octave.
$\frac{4}{5}$ do.	Major 3d. ..	Double Octave of the Major 3d.
$\frac{17}{20}$ do.	Minor 3d. ..	Double Octave of the 5th.
$\frac{3}{5}$ do.	Major 6th ..	Double Octave of the Major 3d.

[The moveable bridge is not used in this experiment.]

Of Discords.

The 2ds and 7ths are *independent* characters, mutually connected with each other, and bearing no relation whatever to any of the foregoing concords.

Such is the undeniable statement of harmonic relations, by which you will readily perceive the *remoteness* of our 3ds, and their consequent rejection by the Greeks, who acknowledged only *two* concords, (the octave being considered as a repetition, or rather as an antiphonious sound,) in contradistinction to the unison, which was termed homophonious.

These two ancient concords were,

The 4th, regarded perfect.
The 5th, regarded imperfect.

Were you to take the trouble of reading Burney's History of Music, you would smile at his unwillingness to acknowledge the 4th as a concord at all. His reason is obvious, the *comparative* perfection or imperfection of our harmonic system being so dependent upon this important fact.

That harmony, according to the modern sense, was rejected by the Greeks, requires but little argument. In every system adopted by that extraordinary people, they sought *perfection*, nor could they by any means consent to the erection of a superstructure upon ever so slightly defective a basis. With them no other combination of tones was held admissible, than that of the unison and octave; and for these they might well have pleaded the sympathy of Nature herself. Aristides Quintilianus, an ancient Greek writer, has related the case, and the proof is readily attainable with our monochord.

Experiment 4. Tune to *perfection*, on the piano-forte, every note within a given octave.

Tune the monochord in perfect unison with the upper note of that

that octave, suppose *c*. Place it on the piano, and balance across the wire a small slip of fine tissue paper, sufficiently bent to sustain itself from falling.

Strike the fundamental C of the piano pretty strongly, and the wire of the monochord will vibrate a *little*; just so as to agitate the paper.

Strike the upper *c*, that is the unison of the monochord, in a similar way, and the paper will be *strongly* agitated.

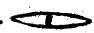
Strike every other individual note between C and *c*, and no such effect shall be produced.

So far for *sympathy*, which fully authorized the adoption of the unison and octave; and so far may the ancient system be defended. But why should that system have been confined to the narrow limits of the disdiapason or double octave?

Perhaps this question may be solved by another. May not the Greeks, whose constant maxim was that of *unity*, have considered the musical string itself as the natural boundary of the musical system, every sound, though apparently simple, being neither more nor less than a compound of the numberless intervals

of the grand system, or whole string .. .. ..

and of the two smaller, or disdiapasons, into which the string while vibrating is divided .. ..



every portion thereof being at the same moment in more or less effective operation—*with*, and, if I may use the expression, at the same time *without* a central bridge?

This conjecture may not be deemed altogether irrational when we reflect upon the ancient character,—thus maintaining the unity of Nature in all the fullness of perfection.

But why did not the Greeks consent to the subdivision of *time*, the only distinctions with them being the long and short, in the ratio of one to two, similar to those of our semibreve and minim, or crotchet and quaver? I answer—The preservation of their beautiful language, whose genius does not even admit our ordinary *barring*. Had the frittering away of syllables been once encouraged, and considered as a musical beauty, where would the innovation have ceased? Ancient Poetry herself would have lost her character, and ancient Oratory have been degraded for ever.

In addition to the previous inquiries, that of the ancient *modes* must not pass unnoticed. Little however can be said respecting them, the necessary materials being irrecoverably lost. They appear to me to have been somewhat analogous to what we now term the different *keys*,—each of which keys, in consequence of the manner of tuning the instrument, had its own fixed character, which character would have been destroyed by transposition into any other key. No general temperament, therefore, could have

answered the design, the soni mobiles, or moveable tones, requiring a new manner of tuning upon every change of mode.

In these changes, the 3ds, no doubt, must have had their influence, flattening the minors adding considerably to the *plaintive*, and sharpening the majors to the *maddening* effect of the composition.

Such niceties could hardly have been discriminated, much less executed, during the grosser ages; in which, according to Burney, the singing of a simple semitone in tune was almost insurmountable. Hence, and hence alone followed what we moderns have been pleased to term "the reformation of the scale." "Guido arose," say a number of musicians who never took the trouble of exploring antiquity: But what did this Guy Aretin achieve? Little more than the improvement of *lines* (for even these were partially adopted before his day), and the total abandonment of tetrachords with all their delicate distinctions; substituting in their place, not the system of octaves, but the less comprehensive one of hexachords or 6ths.

As to harmony, or rather note against note,—for he had no idea of more extensive combination,—the accompanying tones of the 4th, 5th, and 8th were employed a long time before him; the Monk Hubald, who flourished in the tenth century (nearly one hundred years before Guido), having left a sort of treatise on Music, which shows that not only the practice of limited counterpoint prevailed at that period, but also that in addition to the 4th, 5th, and 8th, both 2ds and 3ds were occasionally admitted.

Dr. Burney has given us some particular accounts of *Guido*. Among other singularities, he forbade the use of the 5*th* in harmony, although he frequently employed the 2d and 4th, as likewise the major and minor 3ds, which latter (the 3ds) had for some time been gaining ground.

These, with the cultivation of lines and the abandonment of the tetrachords, as I have already mentioned,—together with the extension of the disdiapason to two octaves and a sixth, assigning, as *some* suppose, the name of G, or Gamma, to the lowest note, from whence the term Gamut,—are the most notable matters recorded of this applauded monk.

With respect to our *time*-table; even in the day of John de Muris, who lived in the fourteenth century, it contained but four or five characters, and was therefore very limited compared with that table which after-ages contributed to extend and improve. Nor can any music be found of the preceding centuries, consisting of more than two parts; and these in the strictest counterpoint of note against note.

Thus every thing was progressive, nor have we any reason to

affirm

affirm that any extraordinary genius arose to whom posterity has been singularly indebted. Even *barring* itself was never practised, at least in England, before the reign of our Charles the First.

Let us now finish our examination of *ancient* music, by inquiring into the more immediate causes of its destruction, as well as the ravages which almost obliterated its very traces.

After the conversion of the emperors, it would appear that all the theatres and other public spectacles were discouraged; and that nothing but the insipid psalmody of the primitive Christians could find its way into the churches and private dwellings. Thus vanished by degrees the Greek and Roman secular music; no private person being capable of executing the refined and difficult music of the theatre. Add to this, the ultimate overthrow of both the Eastern and Western empires; and, not to speak of Gothic ravages, the plundering and burning* of Rome in 1527 by the army of Charles V., by which the records of the pontifical chapel, with innumerable works of every description, were destroyed;—and we shall by no means wonder at the paucity of musical documents which have reached our time.

[To be continued.]

LIII. *Report of the Select Committee appointed to consider of the Means of preventing the Mischief of Explosion from happening on board Steam-Boats, to the Danger or Destruction of His Majesty's Subjects on board such Boats.*

[Concluded from p. 256.]

Mr. Josias Jessop's *Evidence.*

State to the Committee what you are, and where you reside? —I am a civil engineer, residing in the Adelphi, London.

Are you acquainted with steam-boats?—I know the principle of them; I have been on board of them, and seen them.

Our object being to inquire into the method of ensuring a greater safety to the passengers on board those steam-boats, have you any thing to communicate to the Committee respecting that object?—If that were the only object, there can be no doubt that one of low pressure must be more secure than one of high pressure; for although they may be both easily made secure originally, yet from the natural wear and tear, both are liable to accidents. If an accident happen to one of a high pressure, its consequences certainly will be more dangerous than that of a low pressure engine.

* See Burney, who has quoted *Andrea Adami.*

Is it your opinion that a high pressure boiler may be constructed so as to make it perfectly secure?—That is a theoretical question to give an answer to; I should say yes, certainly; but experience proves that both wear out.

What would be the construction, and what the precautions you should recommend, in order to ensure that safety?—That it should be able to withstand the proof of two or three times the pressure to which you are likely to put it, or rather the pressure to which you should be limited; if, for instance, you meant to work it at fifty pounds pressure, and it stand the proof of one hundred-and-fifty pounds, the presumption is, that it is secure; but in the course of two or three years all boilers wear out.

What are the precautions that you would recommend to prevent a boiler being used at a greater power than what it was adapted for?—By having an additional safety-valve, to which the person who works the engine should not have access.

Is there any other precaution that you would recommend?—I think that if it were made of malleable metal, such as iron and copper, it would be an additional security.

What is the ground of your preference to malleable or wrought metal?—It does not burst by an explosion, as brittle metal does, but tears; it would probably rend at the joints.

You do not mean then to say, that it would be impossible that a malleable boiler would burst, but that it is improbable that it would?—It would burst, but it would not fly in pieces; the rent would create a natural safety-valve.

Are you acquainted with the fact, that high pressure steam and water heated so as to raise that steam, do not scald in the same manner with water and steam at the heat of 212°?—I am not acquainted with the fact; but I have no difficulty in believing that the steam will not scald, although I should think that the water will.

Do you think that if the safety-valves be properly adjusted to the strength of the boiler, and so constructed as to work with perfect ease, and one of them put out of the reach of the engine-man, there is any occasion for the additional aid of a mercurial gauge?—I should think not.

Have you any particular suggestions to make respecting the construction of the boiler?—The most convenient form of the boiler is, that it should be adapted to the shape of the boat; and I should think, that that being taken for granted, the safety would depend upon the strength of the metal, and not upon the form. It should be made of such strength, that any indenture would not affect it. Although the form approaching to cylindrical is of course stronger than any other form, that which nearest

approaches

approaches to a sphere is the strongest, but a cylinder with semicircular ends is best; I mean hemispherical ends.

Is it not very possible to burst a low pressure engine, if the engine-man is careless, or rash enough wholly to neglect his steam-valve?—Certainly; I think that they are equally liable to burst, only the one bursts with greater danger and risk than the other.

Mr. ALEXANDER NIMMO's *Evidence.*

What are you?—A civil engineer, and generally residing in Dublin.

Have you any experience of the construction of steam-engines for packets or passage vessels?—I have seen the steam-vessels in the Clyde, on the Thames, and vessels in Ireland, and those vessels lately constructed for passage between Dublin and Holyhead; and I have studied the subject with a good deal of care, in a professional point of view. I have lately been employed by the Dublin Steam Packet Company, to alter one of their vessels, which was not found completely fitted for crossing the sea; I have altered that vessel, and she is now plying in the Bristol Channel preparatory to going to Ireland.

Have you, in consequence of your experience, any suggestions to make as to the safe construction of the engine boiler connected with such packets?—A great part of the alterations that I made upon this vessel, were intended to fit her for going through the waves, and to alter her machinery; and another portion of them was likewise directed to make her safe as to the engine. You are aware that it is necessary for all engines of that description to have safety-valves. The defects of the safety-valve which I altered, were, that it is not now in the power of the engine-man to keep it shut; it is in his power, or that of any passenger, to open it, however, at all times so as to discover whether it be in good order, by a small chain and a weight being within the boiler: it is not in his power to add to it while in action: and lastly, this vessel being intended to go to sea, and to work as she has done, in very rough weather, the safety-valve is made equally effectual in every position of the ship, whether she heel, pitch, or roll. The weight preserves the valve in motion, so as to keep it from sticking, and it has always the same effort to overcome. I will thus describe the nature of the valve: It is a hemispherical cup with its convex surface downwards, resting upon a collar, and to the bottom of the cup a weight is hung which has previously been adjusted; by this means the valve is always steam-tight in every position, yet without danger of adhering, and must be lifted by the steam when it exceeds a given pressure; but the valve may also be lifted by a chain attached to

its upper side, which is inclosed within the iron case, and may be drawn by the engine-man or any person on board, and which does not allow him to keep it down or to confine it. We have also found it necessary to prevent the accumulation of water upon the top of this valve, arising from the condensed steam when escaping; this is done by a small waste-pipe descending from the bottom of the pipe which conveys away the waste steam; it is a waste-pipe for water. I have thought it advisable to make the steam-valves large, and that the weight which is laid on being of itself large may admit easily of addition. I have one or two more precautions to suggest for safety: In this vessel there are two boilers communicating, and two safety-valves; there is also a mercurial gauge provided with receivers, so as to prevent the loss of the mercury in case of any sudden collapsation or disengagement of steam, also a tube of glass attached to the boiler, which exhibits the level of the water in the boiler, and precludes any idea of danger in the minds of the passengers; these boilers are made of wrought iron, but I do not consider them as being better on that account.

Do you think equal mischief is likely to arise from the explosion of the wrought-iron boiler, as from the explosion of the cast-iron boiler?—That depends upon construction.

Put construction entirely out of the question; suppose the form exactly similar, do you conceive that equal mischief is likely to attend the explosion of the wrought-iron boiler, as the cast-iron boiler?—If the construction of the cast-iron boiler admits of its being made of wrought iron with equal strength, then the explosion of the cast iron one would be more dangerous, as it will fly in pieces, whereas the other would probably tear; but it is scarcely fair to stop at this hypothetical case, as we must consider what can be done in practice. It is scarcely possible to form cast iron every where equally strong, and if a part be weaker than the rest, either on purpose or by accident, that will not have the safety that would be obtained by a wrought-iron boiler: for instance, in cast-iron boilers it is common to have holes, and if these be filled with some metal of different melting temperature from cast-iron, more fusible for instance than that, the juncture will part first, and it may be made to tear as a wrought-iron boiler would do; and again, the wrought iron is so much more liable to oxidation than cast iron, that although found very efficient at first, its strength and tenacity may be very speedily altered; for these reasons cast-iron boilers have been preferred where high pressure engines have been used, and in small tubes the tenacity of cast iron can be made greatly to exceed that which can be given to wrought iron in the same form. I believe all large boilers have latterly been made of wrought iron,

iron, as it is difficult to make them of cast iron. Although no friend to high pressure engines in vessels, nor to cast-iron boilers, on account of the danger of explosion, yet I conceive the chief danger of that kind is likely to arise from working low pressure boilers at a higher pressure than they were intended for; and I conceive that the principal improvement to be looked for hereafter in steam vessels, is, to simplify the machinery, and put it in less room, which the high pressure engine and cast-iron boiler afford us the means of effecting, and the other does not. I will state another thing as of consequence, viz. as to preventing a vessel taking fire; it is advisable that the furnace and flues, if not entirely above the deck, should at least be inclosed in a case of water or other non-inflammable matter, until they arrive above the deck. This precaution I strongly recommend to be adopted.

Mr. Arthur Woolf's *Evidence.*

What are you?—I am a civil-engineer in the village of Pool, in the parish of Illogan, in the county of Cornwall.

Are you conversant at all with steam packets?—No; I never had any thing to do with steam packets; they are out of my line.

You have been long acquainted with steam-engines?—Yes.

You invented the one that goes by your name?—Yes; I got a patent for that steam-engine.

Have you any thing to communicate to this Committee, as to the object upon which we are met, which is, the safety of steam-engines and boilers on board steam-packets?—With the boilers I have been in the habit of using for fourteen years, we never have had any accident at all.

Of what are they constructed?—Of cast iron.

Are your boilers in general made of wrought or cast iron?—Of cast iron wholly; I approve of the cast-iron boilers in preference to any mixture of metals.

Do you consider that the cast-iron boiler, upon the common construction, is equally safe with a wrought-iron one?—Not upon the common construction that I have seen; some I should have doubted very much; I have seen some that are rather dangerous; my patent consists of one composed of a number of tubes.

What is the difference between your construction of the boiler and the common construction, which, in your opinion, renders yours so much safer than the other?—It is always necessary in boilers to have a certain quantity of surface exposed to the action of the fire, to contain heat and steam; and if that be done in one vessel, of course it must be of considerable size, greater in

in diameter than if composed of a number of tubes; and the risk of explosion is in proportion to its quantity of surface.

Do you mean to say that, generally?—With the same pressure, certainly.

Would your boiler be as well adapted for a steam-boat, as those generally made?—Yes, they are calculated for every purpose; they are generally adapted to high pressure steam; my patent was taken out for a safe boiler for a high pressure engine; indeed, in my own engines, I do not work the steam to that height as is done in what is called the high pressure engine, as the novelty of my engine is, that I work the steam twice over.

What precautions do you take to prevent accidents?—I always make my boiler to stand from 14 to 20 times the pressure that I ever make use of.

What precaution do you make use of, to prevent a greater pressure?—The safety-valve is what we depend upon; I always apply two safety-valves, as I have seen incidents where the valve has accidentally stuck fast and would not act; and I have a safety-valve, of a particular construction, that never can stick fast.

Do you use mercurial gauges?—Never for a safety-valve; I never found it necessary to have one, not for an escape.

Have you any other precautions besides those you have mentioned?—Not any, but as to trying boilers to see that they are strong enough; that is the point that I recommended in my specification, that they should be proved by pressure every time the boiler is emptied for cleansing; then to fill up the boiler with cold water quite full, and put an extra load of five or ten times the power of steam; and then, by a forcing-pump, to syringe water in till it lifts the valves; then there can be no danger, there can be no explosion.

Suppose a cast-iron boiler, and a wrought-iron boiler of about the same form and capacity, to be exploded by the force of the internal steam, do you think that the mischief likely to be produced by each of those, would be equal? taking any form you please, and exploding both, which would do the most mischief?—I do not think the wrought-iron boiler would separate into so many pieces as the cast-iron boiler.

Then do you think that the explosion of the wrought-iron boiler is attended with as much danger as the cast-iron boiler?—In every thing, excepting what depends upon the fragments of the iron itself; I have no hesitation in saying, that cast-iron boilers are safer than wrought-iron boilers.

Why?—Because we can make them of a greater strength; you cannot make a wrought-iron boiler so strong as a cast one.

For high pressure you may have a boiler of cast iron stronger than

than you can of wrought iron?—I can make it stronger and more to be depended on for great pressure; but where great pressure is not wanted, wrought iron can be made sufficiently strong to depend on.

Supposing an accident happened, would not a wrought-iron boiler be attended with as great mischief as a cast-iron one?—As great a number of accidents happen from the common boilers or wrought-iron boilers, as from the cast ones.

Mr. ANDREW VIVIAN'S *Evidence.*

What is your profession?—Miner and engineer.

Where do you reside?—At Cambourne, in Cornwall.

Have you been long acquainted with the construction of steam-engines?—For thirty years and upwards.

You are then capable of giving an opinion as to those circumstances by which danger is occasioned in the working them, and the means of preventing it?—I am.

Be pleased to state them?—The danger arises from making the steam-vessel of insufficient strength for the steam; every engineer ought to be well acquainted with the power of the steam, and make the steam-vessels in proportion to the strength of the steam required.

What precautions do you use to prevent explosion?—Safety-valves; not less than two on every boiler where a high pressure of steam is required, and that the boilers be made of sufficient strength, and proved before used.

To what proof are those boilers subjected, or to what proof ought they to be subjected?—By filling them with water, and loading the safety-valves with, perhaps, ten times the weight required for the engine, and then by injecting water into them, so as to lift those valves with ten times the weight required.

When you say ten times, do you mean exactly ten times?—Perhaps, ten or twelve times the weight it is intended to work.

You conceive that a boiler which has been so proved and furnished with safety-valves, properly adjusted to its contents, to be perfectly safe in working with steam, whether high or low pressure?—Yes, I do.

Is there any difficulty in so adjusting the apertures or valves; that is, in calculating of what size the valves ought to be, to produce safety in a boiler of any given magnitude?—No, no difficulty at all; it is a plain and well-known thing to all engineers, or to every one who ought to pretend to be an engineer.

Is it usual to work a high pressure engine at all near to that point to which the valves are thus adjusted?—We load the engines in the mines under my directions, to about forty pounds an inch; and the valves are then loaded to about forty-five, perhaps.

But

But those valves are capable of being loaded up to the full extent to which the engine has been proved?—Yes.

Is it not easy and common so to construct one of the safety-valves, of which you have spoken, as that the engine-man shall not be able to load it beyond the pressure intended?—One may be locked up, and very easily kept from the engine-man.

When that one is so locked up and kept from the engine-man, is it possible, according to the common calculation of events, that the boiler should then explode if the valve works freely?—I do not conceive it possible that it could.

Is it not easy so to construct the valve as that its operation shall not be hindered by any accident, such as adhering to the sides or clogging from fouling, or any thing of that sort?—Very easily constructed, so as not to be liable to those accidents.

Do you then see any reason why, in any situation whatever, the use of an engine should be limited to the low pressure, or that which is usually called the condensing engine?—By no means in the world.

Do you conceive that there is any difference in the liability to explode, between the boilers constructed of wrought and of cast iron?—I should conceive that cast iron could be made much stronger than wrought iron, with less difficulty; I conceive it to be a very difficult thing to make a wrought-iron boiler so strong as we can have it cast: we have some of our boilers made two inches thick; and to make a wrought-iron boiler equally strong as that, would be very difficult to be accomplished by workmen.

Supposing a cast iron and a wrought iron to be of the same form, and each of them to be exploded by too great an internal force being applied, which of the two do you think is likely to produce the greatest mischief in the explosion?—Certainly, a cast-iron boiler is likely to separate into more parts than a wrought may be, and is likely to do more mischief.

What accidents have happened to steam-boilers within your own knowledge, working either with low or high pressure steam?—I have known of no accident with high pressure steam and cast-iron boilers; but I have known an accident happen working with Boulton and Watt's low pressure engine, which was on the 28th of November 1811, in Wheal Abraham mine; a wrought-iron boiler working with low pressure steam exploded there and scalded six men, three of whom died in the course of a week afterwards.

Were any persons at that time killed by the fragments of the iron?—No; it was entirely by the steam and the water.

Do you recollect any instance in which a wrought-iron boiler exploded, so as that any persons were killed by the fragments?—I do not.

Do

Do you at all know, whether there is any difference between the power of the steam or water to scald, when under high or low pressure?—The steam from low pressure scalds much worse than the steam from high pressure; as to the water, I cannot say. I cannot conceive that water can issue to any great distance from a high pressure boiler, it must soon be steam; it must be converted into steam from its heat; water cannot go beyond 212 degrees of heat, unless it is confined; beyond that it must be steam.

Have you ever known any person scalded by the steam or the water issuing from a high pressure boiler?—No.

Have you ever known any instances of persons being scalded by the steam or water from a low pressure one, besides that which you have mentioned?—I have heard of a great number of instances of this in different mines, but only that one came directly under my own eye.

You have given the Committee to understand, that when boilers made of wrought iron are exposed to steam of high pressure there is great difficulty in making them sufficiently strong, for that the rivets are apt to draw, and the joints to become loose; do you not conceive it very possible for the boilers in such cases to become useless by permitting the steam to escape, and yet not to explode so as to produce any fatal accident whatever?—I am quite of opinion, that boilers made of wrought iron for high pressure engines will soon become leaky, and may not explode. I have known of an instance of a boiler of that description made for Herland mine, and it soon became leaky, and unfit for use in a very short time; the consequence of which is, the mine is knocked up, and a great number of people out of employ.

But no accident happened in consequence of this, by which any person was injured?—No.

Supposing the only object to be safety to the lives or limbs of the persons who should be surrounding the engine, would you in that case prefer having the boiler of a high pressure engine of wrought or of cast iron?—I would have cast iron, because it can certainly be made stronger than wrought iron for the same expense.

Do you take into that calculation the difference of the mischief which might be occasioned, supposing the boiler by any accident to explode?—I consider that risk is so small as that it scarcely need be taken into the question, because all explosions may be easily prevented by proving the boiler every time it is cleansed, which I think should be at least every month.

Is the operation of proving the boiler so easy as that it may be performed on board a steam-boat every month without expense

pense or inconvenience?—Certainly; it consists, as I have said before, only in filling the boiler with cold water, putting great additional weight upon the safety-valves, and then injecting water by a forcing pump till the valve is lifted with the additional weight.

Is it then your opinion, that every steam-engine ought to be provided with those means of proving the boiler, and that they ought to be applied as often as you have mentioned?—Yes, they should; the very pump by which the boiler is supplied with water may be used conveniently and sufficiently for this purpose; high pressure boilers are all fed in the common way by forcing-pumps, and there is no difficulty in applying it to the low pressure boilers.

Have you found the use of a high pressure engine of any advantage to the Cornish mines?—Of very great advantage, which can be proved by looking at the monthly reports, which I am fully convinced are correct.

Do you conceive that a low pressure engine can be reckoned entirely safe, unless furnished with the safety-valves, such as before described, and to one of which at least the engine-man has no access?—I conceive that every engine ought to have those valves; and one should be locked up to prevent careless engine-men doing mischief, which low pressure engines are as liable to as high.

Have you any means of estimating the comparative consumption of coals by either high or low pressure engines?—Yes.

Be so good as to state your opinion upon that point?—I conceive that a high pressure engine does greater duty with the same coals than a low, which will also be proved by the monthly reports.

Have you any further information on this subject which you wish to communicate to the Committee?—Being desired to attend here on the part of the proprietors of three of the largest mines in Cornwall, the united mines of Crowan, Dolcoath and Wheal Unity, I wish to state their hope, that the Legislature will not interfere to prevent the use of high pressure engines, either on board boats or in any other way.

You have not been used to steam-engines on board boats, but in mines chiefly?—Chiefly in mines. I have seen them on board boats.

Do your answers apply equally to steam-engines on board boats as in mines, or may not more caution be necessary in boats than in mines?—I conceive the answers to be applicable to boats as well as to mines.

LIV. *On*

LIV. *On extracting Alcohol from Potatoes, and preparing Potash from Potatoe-tops**.

THE simple process described in the following communication deserves to be made very generally known, as a source of considerable emolument to the growers of potatoes.

On extracting Alcohol from Potatoes.

A French lady, the Countess de N***, whom political events compelled to change her chateau on the banks of the Saone for a cottage eight leagues from Vienna, has established, on the small farm she occupies, a distillation of brandy from potatoes, which she has found to be very lucrative. The brandy of twenty degrees of Reaumur is very pure, and has neither taste nor smell different from that produced by the distillation of grapes. The method she employs is very simple, and within every person's reach.

Take 100 pounds of potatoes well washed, dress them by steam, and let them be bruised to powder with a roller, &c. In the mean time take four pounds of ground malt, steep it in lukewarm water, and then pour into the fermenting back, and pour on it twelve quarts of boiling water; this water is stirred about, and the bruised potatoes thrown in, and well stirred about with wooden rakes, till every part of the potatoes is well saturated with the liquor.

Immediately six or eight ounces of yeast is to be mixed with twenty-eight gallons of water, of a proper warmth to make the whole mass of the temperature of from twelve to fifteen degrees of Reaumur;—there is to be added half-a-pint to a pint of good brandy.

The fermenting back must be placed in a room, to be kept by means of a stove at a temperature of fifteen to eighteen degrees of Reaumur. The mixture must be left to remain at rest.

The back must be large enough to suffer the mass to rise seven or eight inches, without running over. If, notwithstanding this precaution, it does so, a little must be taken out, and returned when it falls a little: the back is then covered again, and the fermentation is suffered to finish without touching it—which takes place generally in five or six days. This is known by its being perceived that the liquor is quite clear, and the potatoes fallen to the bottom of the back. The fluid is decanted, and the potatoes pressed dry.

* From the Monthly Magazine for October 1, 1817. Communicated by Mr. B. Jones of B[illegible].—The additional experiments appeared in the Dublin Journal of the 25th of October, and were probably undertaken to verify those of the French chemist.

The distillation is by vapour, with a wooden or copper still on the plan of Count Rumford. The product of the first distillation is low wines.

When the fermentation has been favourable, from every 100 pounds of potatoes, six quarts and upwards of good brandy, of twenty degrees of the areometer, are obtained; which, put into new casks, and afterwards browned with burnt sugar, like the French brandies, is not to be distinguished from them.

The Countess de N. has dressed and distilled per diem 1000 pounds of potatoes at twice, which gives sixty to seventy quarts of good brandy. We may judge from this essay what would be the advantages of such an operation, if carried on on a grand scale, and throughout the year.

The residue of the distillation is used as food for the stock of her farm; which consists of thirty-four horned cattle, sixty pigs, and sixty sheep: they all are excessively fond of it when mixed with water, and the cows yield abundance of milk. The sheep use about five quarts per diem each; viz. one-half in the morning, and one-half at night. The malt must be fresh-ground: the Countess has it ground every week.

On the Means of extracting Potash from Potatoe-tops.

One of the most important discoveries of the present day is that of a druggist of Amiens, by which Europe will be freed from the heavy tribute she pays to America for the article of potash. The author of this discovery has, in a truly patriotic manner, made known his discovery—after ascertaining, by a series of experiments, the truth of his conclusions. The French Society of Agriculture, and the Society for Encouragement of National Industry, have both named commissioners to frame official reports: in the mean time, we feel it important to give an account of the process, in the hope that, even in the present season, it may be turned to account—as it interests landlords, tenants, merchants and manufacturers.

It is necessary to cut off the potatoe-tops the moment that the flowers begin to fall, as that is the period of their greatest vigour; they must be cut off at four or five inches from the ground, with a very sharp knife. Fresh sprouts spring, which not only answer all the purposes of conducting the roots to maturity, but tend to an increase of their volume, as they (the sprouts) demand less nourishment than the old top. The tops may be suffered to remain on the ground where cut; in eight or ten days they are sufficiently dry without turning, and may be carted, either home or to a corner of the field, where a hole is to be dug in the earth, about five feet square and two feet deep (the combustion would be too rapid, and the ashes cool too quick,

quick, and thereby diminish the quantity of alkali, were they burnt in the open air). The ashes must be kept red-hot as long as possible: when the fire is strong, tops that are only imperfectly dried may be thrown in, and even green ones will then burn well enough.

The ashes extracted from the hole must be put in a vessel, and boiling water be poured upon it, as then the water must be evaporated: for these two operations potatoe-tops may be used alone as firing in the furnace, and the ashes collected. There remains after the evaporation a dry saline reddish substance, known in commerce under the name of *salin;* the more the ashes are boiled, the grayer and more valuable the *salin* becomes.

The *salin* must then be calcined in a very hot oven, until the whole mass presents an uniform reddish brown. In cooling it remains dry, and in fragments—blueish within, and white on the surface; in which state it takes the name of potass.

The ashes, exhausted of their alkaline principle, afford excellent manure for land intended to be planted with potatoes.

The following is a table of the results obtained in France:

An acre planted with potatoes, at one foot distance, gives plants	40,000
These 40,000 plants yield, on an average, three pounds per plant at least, or of green tops ..	120,000 lbs.
On drying they are reduced to	40,000 lbs.
This quantity produces of ashes	7,500 lbs.
The evaporation gives of ashes exhausted of alkali	5,000 lbs.
Salin	2,500 lbs.
The salin loses ten to fifteen per cent. in calcination, which gives of potash	2,200 lbs.

All these estimates are taken at the lowest; by which it is evident that upwards of 2000 pounds of potash may be obtained, in addition to an increased crop, from every acre of potatoes, or a value far exceeding that of the crop itself. Farmers of course will next year turn this discovery to the best account, in planting those potatoes which yield the greatest quantity of tops. The expenses of preparing the potash, as above described, including every thing, is about six guineas per acre.

*** I cannot conclude these articles without inviting the cultivators of England and Ireland to instantly seize the immense advantages afforded by the two discoveries here announced. The former will free us from our tribute to France for brandies; a commerce which the Emperor Napoleon turned to such good account during the war—insisting on British vessels, which carried over staple commodities to France, to return with cargoes of wine and brandy; and the latter will, it is trusted, free commerce, and our dyers in particular, of the necessity of applying

 to

to Russia and America for potash, of which our consumption is immense. I will, in an early number, give the French methods of making the best brandies, which I collected in the same capital.

Additional Experiments.

About the 20th of last September, Mr. Rice, of Trinity College, cut down a quantity of the stalks of the apple potatoe, which being carefully dried and burnt, and the ashes lixiviated, evaporated, and (in part) analysed, gave results from which he concludes that 1000 parts of green stalk yield of stalk sufficiently dry for burning, 102.87894, producing 7.96088 ashes, affording 3.90313 saline products.

The produce of salt, per Irish acre, planted lazy-bed method, about 245 pounds troy (201 pounds 8 ounces avoir.).

He obtained from 100 parts dried stalk, by incineration, 3.905130 ashes—

From 100 ashes, $\begin{cases} 47.327 \text{ insoluble and earthy matter.} \\ 3.619 \text{ carbonaceous matter.} \\ 49.054 \text{ soluble in water.} \end{cases}$

100.000.

From the soluble part, concentrated in an iron, and evaporated to dryness in a silver dish, in 100 parts—

8.078 potash.
66.437 muriate of potash.
10.807 sulphate of potash.
6.400 carbonic acid.
4.900 water.
3.000 earthy and metallicc arbonates and silica.
99.622

Together with a small quantity of alumina, and a minute portion of iron, the exact weights of which are not yet ascertained. The potash is of a dark-gray tint, its solution very much coloured by extractive, and exhibiting traces of manganese.

To enable our readers to form a judgement of the relative produce of potatoe-stalks, we lay before them an estimate of the produce of a few trees and vegetables, calculated by those whose names are annexed:

	Botanic Names.	Engl. Names.	Ashes.	Saline Products	Authorities.
1000 Parts of dried	Salix cinerea - - -	Sallow -	28.00	2.85	Acad.Sci
	Pinus sylvestris - -	Fir - -	8 40	0 40	
	Sonchus arvensis -	Sow Thistle	105.00	19.66	Pertuis.
	Fumaria officinalis -	Fumitory -	219 00	79.00	Wiegtel.
	Artemisia Absinthium	Wormwood	97.40	73 00	Ditto.
	Urtica dioica - - -	Nettle -	106.70	25.00	Pertuis.
	Erica vulgaris - -	Heath 100 Ashes	. .	11·50	Wildenhein
	Pteris aquilina - -	Fern - -	50.00	6.20	Pertuis.
		Ditto - -	36.40	4.20	Home.

Not having the weight of green stalk subjoined, is a great impediment to calculations, as to the quantity likely to be produced from a certain portion of land. However, the succulent plants, as fumitory, generally lose by drying 8-10ths or 9-10ths of their original weight: it is probable, therefore, that 1000 parts green fumitory produce about 15.8 salt; 1000 parts green fern produce about 14.33 ashes, or salts 1.65.

LV. *On the Physiology of Vegetables.* *By Mrs.* Ibbetson,

To Mr. Tilloch.

Sir, — My general task on the dissection of plants has been for some time suspended by the extreme desire I feel of showing how highly serviceable this sort of knowledge may become to gardening and farming. My usual communications, showing the nature and form of vegetables in general, have therefore been suspended while I was writing and publishing a pamphlet on some of the most important points of agriculture; which are assuredly taught and enforced by phytology far beyond even my first expectations; since there is scarcely a single dissection of plants that may not be said to teach a lesson respecting their management, to enforce a precept most salutary to farming in general.

But I now return to that matter of which I am daily and hourly receiving stronger conviction.—I have shown in the most plain and perspicuous manner in my power, that what has been taken for the perspiration of plants is, on the contrary, matter drawn from the atmosphere and communicated to the vegetable; that perspiration is as unnecessary as impossible to a cold-blooded being like a plant, since it is shown to be perfectly incompatible to the formation of even a cold-blooded animal; that the figures taken for perspiration are so completely unlike a bubble of water, that no person seeing them could possibly deny that a gross error must have arisen in the science to acquire for such appearances such an appellation, since no one could expect to find perspiration fixed on a pedestal, which all these apparent bubbles are. I have also shown that though to give out water from the plant no sort of mechanism is necessary, yet to draw it in from the atmosphere, and arrange it in the plant, great contrivances are required, besides a powerful vacuum, valves, &c. &c.: and this at once accounts for the mechanic power of the hairs, and the wonderful variety of the figures and appearance of these instruments. I have before shown that plants can be fed only in two ways;—by means of nutriment from the *atmosphere*, and from *the earth;* and that in whatever situation the plant *is*, whether at the *commencement* of its formation, or

conclusion, this *nutriment* is so *bestowed* as to be yielded to it in so visible and *evident* a *manner*, as always (if the vegetable be laid open) to be apparent to the eye, and clearly and easily understood by the mind, by the help of a small half-guinea magnifier:—whether it is the bud, the flower, or the seed, they are *all nourished* nearly in the same manner, but *at different times*; and the whole process to be traced by the eye, and when it is the nourishment of the earth, to be followed from the *root upwards*—if the dissector will take the trouble to investigate it: but in the seed-vessel it is peculiarly so. I have shown how the little heart of the seed is formed in the radicle, and carried from the root up the alburnum in the stem, and from thence to the seed-vessel at the bottom of the bud. (See fig. 1, Plate V.) I shall now explain in how curious a manner part of the seed is filled up by the atmosphere, and afterwards by the earth.—This is exemplified in no plants in a better or plainer manner than in the wheat and gourd. The nutriment is first bestowed by the atmosphere: the mechanism is then so wonderful and so beautiful, that the dullest being would be struck with admiration and astonishment, if viewing the graceful feather that performs the operation. (See fig. 2. Plate V.) After fructifying the plant and partly filling the heart, the juices of the atmosphere fill up the bag of the seed more than one-third, before the vessels that run up from the root open their mouths to bestow their quota of nourishment which is collecting in *the pith* for the purpose: —But all this the print will best explain.

As soon as the males or stamens appear hanging out of the scales, the wheat must be taken: it will then be found that the *heart* of *the seed* (fig. 2. *a*, *a*), has just entered the bag of the seed, which is then (except the corculum) empty, or inflated with air only. As soon as the heart (*a a*) is fastened at the top of the bag, all the beautiful mechanism appears from which the nutriment of the atmosphere will arise: at that time the pollen spreads all over the feather of the stigma, dissolves when mixed with the sweet juice it there encounters, and runs down the feather for the impregnation of the seed:—it may be seen at fig. 4, of what regular pitchers the feather is composed, for its reception. As soon as the impregnation of the heart of the seed is completed, then the feather cleanses itself from the pollen, and the pitchers which had before been so filled with the sweet juices of the line of life, as not to admit the moisture continually falling from the heavens all around it; but the sweet juice now disappearing, and the pitchers being empty, open again to receive the juices of the atmosphere, and the bag of the seed begins visibly to fill. But it is not the feather alone which conveys nourishment from the atmosphere; the quantity of hairs which surround the grain of

of wheat, and are in such numbers as completely to conceal and cover it (see fig. 2. *bb*), pour in their nutriment at the bottom of the bag (*c c*); but it equally rises to replenish the seed:—the entrance of the two juices is, however, easily distinguished; they are both liquids, though that which proceeds from the feather is infinitely thicker and richer than the other, which appears like water, though with much gas in it. As soon as the feather and hairs have completed their absorption, and that the whole has entered the bag of the seed, the apparatus dies away by degrees; and all that graceful sparkling feather, with its attendant hairs *on each seed of the wheat*, all vanish, or leave such a trifling vestige as to show that it is no longer of use. The whole appearance of the plant becomes *altered within*, from a number of vessels which almost fill up the interior; sometimes there are as many as eight; they run up to the flower-stem within the pith, and the stem of course swells and enlarges to begin the next process, which is the yielding the nutriment from the earth. (See fig. 2. *d,d,d.*) I would not give two figures of the wheat. To show therefore this part of the process, I have taken the gourd, because it makes the filling up of the bag of the seed still plainer than it could have been in the wheat:—this plant having many seeds in one seed-vessel, it more easily shows the nutriment flowing up the pith into each bag from the root, and entering the seed-vessel. See fig. 3. *e*, *e*, *e*, a specimen of the gourd cut horizontally, and showing the various seed-vessels already half-filled by the atmosphere, and beginning to take in their nourishment from the earth. The whole process is exactly the same as in the wheat; only that the vessels, which were six in number, and which poured in the nutriment from the atmosphere into the seed-vessel, have all disappeared, and the pistil has dried off too much to send any more juices to the part; for the gourd equally took in the atmospheric juices from the stigma and the watery nutriment from the hairs that for the time surrounded the seed-vessel. The vessels *f f* contain the heart of the seeds, and the powdered nutriment is plainly shown at *g*, *g*, *g*, to pour into the bottom of the seed-bag, to which they are fastened, and afterwards detach themselves when complete. I have laid them open merely to show the powder flowing in:—this nutriment may be traced running up from the root in any part of the stem adjoining the pith, provided the exact time of its flowing is taken, and the stoppage only takes place when the seeds are quite full; then the vessels quickly disappear, and form new matter. No mistake can be greater than to suppose the plant is always the same in the interior; but the changes are so quick and immediate that they must be watched for, to be discovered.

The cutting the specimens thus lays open all the vessels, and

of course explains the different process to which they are subject. When the powder is mixed with the liquid in the wheat seed, they soon form a species of thick milk, which as it *hardens* becomes flour: this flour may be traced from the *root upwards*, *in all the vessels formed* for *its reception* in the pith which it surrounds; indeed it often almost engrosses the hollow stem for five or six weeks preceding the cutting of the corn.

Thus then the manner of receiving the nutriment from the atmosphere is totally different from that of receiving it from the earth;—it is impossible to mistake them. It is also equally unlikely to take the nutriment bestowed by the earth (which is an extremely fine powder) for the hearts of the seeds, which are regular balls. I have shown that *a a* are the hearts of the seeds in the gourds. But there is never but one corculum to each seed; and yet the amazing number of hearts are (like the seeds) scarcely to be *reckoned:* Nature's astonishing prolific powers in all these respects are quite wonderful. But one circumstance (as much as any thing I have before mentioned) proves that it is the *heart* of the *seed* which is formed in the radicle which enters the seed-vessel, since (though it enters singly) it has still that string accompanying it with which it is always found; and which soon runs up the pistil, and forms, by enlarging the very vessel which conveys the juice from the stigma to impregnate the seeds, and which never enters but a single heart to each seed: and though I have dissected many thousands, I have never yet found two corculums: though several will come to the edges of the stalk (fig. 3. *h*, *h*, *h*) yet it is only in case the heart has died away, which often happens, that there may be another ready to succeed. As soon as the case of the seed begins to form, all the remaining hearts agglutinate and thicken; and, by degrees forming a mass, complete the outside of the seed. They are therefore of use; there is no loss: and though Nature is so prolific, yet not a single ingredient is wasted. The over-abundance of seeds, if completed, have each their order of insects they were intended to nourish, or for which they were disposed as nests; and the hearts of the seeds (if superabundant) contribute to form the exterior of the seed. Attention to usefulness is peculiarly seen in the chesnut:—there are always six seeds formed in every case, when first the flower appears; but there are never hardly more than two together, when the seeds are completed: when the seed-vessel has received the hearts intended for each seed, it begins to take in the nutriment from the atmosphere; but soon it is found to select two only, and the others by degrees pass off or conglutinate into the mass which surrounds it, while the powder from the root enters afterwards and serves to fill up and nourish the two best seeds. But no different sort of matter can

form

form the important ingredients that come from the root. The heart must always proceed from the radicle, and be formed there, and running up the alburnum into the seed-vessels, but doing so it is subject to get bruised and injured: therefore, when it is so, another immediately supplies its place before it is fructified, but not till the heart has got into its right place. It appears to go through some trial, which not sustaining, it decays and disappears, and another takes its place. I have known three banished thus, one after the other; but they never pass further into the bag than *a, a, a,* but dry away and disappear, while another (the first in the line), *h, h, h,* fig. 3. *takes its place*; and the others in the stalk in time form other vessels required. The corns and grasses are the only plants whose heart of the seed moves after once entering the case. In wheat it first enters at the top; and when it has taken all its nutriment from the atmosphere, it falls to the stalk of the seed (*d,d,d*) and is there fixed firmly and remains stationary. The reason why the *Gramina* differ from all other plants in this respect is, that the seminal leaves are at the bottom instead of the top of the seed, and the heart must be placed close to them, or it could not take in its oxygen; and as it is then (as leaves are ever) the lungs to the young plant, it is constantly supplied not with food but with oxygen from the seminal leaves. This at once proves also that I am right with respect to that point: indeed, unless botanists will dissect progressively and daily, at least for two or three years, it is quite impossible they should not continually take one part for another, one ingredient for the one resembling it.

I have for the last two months watched from day to day this curious process in the gourd and the wheat, and followed each plant from the moment the bud began to appear till it had completed its seed; and each day was a different picture. If then the process changes so perpetually in the interior, how is a phytologist to know any thing of the matter, if he will not look into the plants continually? the exterior is the mere finish of the process; without *this interior* I should never have acquired the knowledge I have gained of the manner in which plants form; or feel so sure of being able thoroughly to depend on the facts I advance in all this varied and complicated picture. If I had not exactly copied from Nature, would not the facts long ago have contradicted each other? Their very consistency proves their truth. Thus then, after all the trials I have made, all the dissections I have observed and drawn, and the constant watching and study of upwards of seventeen years, the whole result of the *regulation of plants is this:* That the root is the *laboratory of plants;* that the vessels which run up being *too small* to admit *whole seeds, whole flowers—all these parts are commenced,*

commenced, and begun to be formed, *in the root*, and are then carried up to the part assigned for their completion; where the ingredients necessary for their nutriment are conveyed and sent up to finish them, before they are obtruded at the exterior of the tree or plant. I have just shown a proof of this sort of formation, the finishing of the seed and seed-case, by means of the nutriment bestowed by the atmosphere, and afterwards from the root, mounting into the seed-vessel, the place appointed by Nature for the completion of the seed. In the same manner the female flowers are formed in the root, then mount, and are completed in the flower-bud, when the seed-vessels are filled with the seeds and the stamen with pollen. The leaves also begun in the root (though so very diminutive), when they quit it, remove to the *leaf-bud*, receive their nutriment from the heavens, and a deep yellow powder from the root. It is not then in one matter only, but in *every ingredient* in the plant, that this fact is exemplified and proved. But it will easily appear that I can know this only by watching daily, or many of the peculiarities would escape me. Thus all the different parts, though they have but one general laboratory, yet they have each a separate part of the plant in which their different ingredients are completed. Is it possible to conceive a more beautiful and more perfect system? the interior of a tree is the whole year as busy as an ant's nest, for every ingredient is in perpetual motion to form and bring forward the earth's produce:—this also admirably accounts for why the root has double the number of sap-vessels the stem has; this is the case in every sort of plant; and it is probably to provide for the formations in the laboratory. Half the sap is allotted to the formation of the flower, seeds, leaves, and nutriment; the other half to the increase of the size of the tree. The more therefore I proceed, the more just I find the thirteen axioms I gave at the (almost) beginning of this work: every set of dissections seems to add to the proof of one or other of them; and it can scarcely be denied that this serves (with other reasons before given) to show the justness of two of them: First, that the root is the laboratory of all plants; Secondly, that there is no perspiration in plants. *These* are *also* some of the plants said to *perspire so much*, merely because the hydrogen is always forced from a plant the moment the vegetable is confined, and that meeting the oxygen which had just quitted the vegetable, and not being able to escape from the glass, they unite again and form their original ingredient—*water*, on the cold glass which attracts them. If the leaves perspired, would they not be constantly wet? whereas they are ever *dry*, *lucid*, and *clear*, except when dew or rain has just fallen. If liquid protruded from the *leaves* (the juice of plants is always glutinous), would not this fix

fix the dust and dirt on them? would not the drops thicken and agglutinate, and run over the plant, marking it with strings of dirt and filth? If the drops issued from the leaves, could the dust be blown off as it may, leaving the plants shining and beautiful, and its vivid green such as to delight the eye and vivify the heart? Conceive how different Nature would appear: How lovely is the clear animated green in spring, when the delicacy of the colour is such as would exhibit every defilement; and yet, even in dusty roads the trees soon throw off, by their motion (and the constant action of the spiral wire) *that dust* which oppresses them, and which the wind helps to disperse:—we are little aware how many important avocations of Nature would be stopped by *such a perspiration*. It is the leaves which receive the nutriment from the atmosphere:—but if that defilement remained glued on the leaves, how could they take it in? it would be absorbed by the dirt;—and what then is all that beautiful glittering net that covers many leaves,—what that waxy covering so conspicuous in all evergreen leaves?—How will that agree with perspiration?—But it is in vain to pursue a theme where all the reasons are on one side alone, and no answer is made but a simple No. Would a plant in-doors collect dust like any other furniture, and that dust be blown or brushed off with a feather? No, it would stick to the leaves. Every one knows what the honey-dew is, though formed by insects which conceal a few eggs in each glutinous bubble; yet it has in reality exactly the effect that would ensue if leaves perspired; for being a parcel of glutinous balls, they collect the dirt and dust, and the dampness of the leaves covers them with a sort of black Cryptogamia, and the whole plant soon becomes disgusting to the sight, and exactly the same as all plants would be if they perspired.

I am, sir, your obliged

AGNES IBBETSON.

P. S. It may also be easily proved that that visible perspiration talked of, and found now and then on a few trees—and which I have for the third time thoroughly examined this year,—is the transparent egg of a small insect, which is at the time feeding under the leaf, while the eggs are left on the upper surface. It is really a matter worth watching—for they have a phænomenon observable, (I believe, just before the insects make their appearance,) which well deserves being examined. It is a most astonishing rotatory motion in the eggs, which running round with great velocity continue to do this for above a minute at a time; nor was it when the mother insect was near: I have shown it to many.

De-

Description of the Plate.

Fig. 1. is the germ of a gourd when the flower and seed-vessels are first beginning to form.

Fig. 2. is the mechanism belonging to every single seed or grain of wheat, and intended to furnish nutriment to the seed both from the earth and atmosphere: but this curious display belongs to the atmosphere principally. (*a*) is the heart of the grain when it first enters the almost empty bag of the seed *b*, *b*, *b*, which is now only inflated with air. The feather *o o* conveys the pollen to fructify the seed, and then takes with the hairs *b b b* the nutriment from the atmosphere, while the vessels *d d d* afterwards supply the powdered nutriment from the root.

Fig. 3. is a specimen of the gourd (before the flower passes off) cut horizontally, to show how the nutriment is principally taken in from the root and pours up the vessels (formed for the time) in the pith, and thrown into the seed-vessels by means of the pipes *c c c*, which are seen to give a fine powder, and thus fill each seed-vessel:—cut them which way you will, they equally yield the same picture.

Fig. 5. is the natural size of the wheat seed, when beginning: it is of no use to try it when older, the process is then over:—when once the seed is ready for the earth, a new process begins: all then is forming for the new embryo, which is indeed even at this time preparing in the *heart of the seed*. But I thought it would make a confusion to show its curious mechanism here.

Fig. 1. shows the gourd cut perpendicularly, with the seeds properly placed, receiving the juices of the atmosphere.

LVI. "*Della Purificazione del Mercurio Memoria del Sig. Dott.* G. BRANCHI," &c.—*Memoir on the Purification of Mercury. By Dr.* JOSEPH BRANCHI, *Public Professor of Chemistry in the University of Pisa, Corresponding Member of the Royal Academy of Science of Pistoja, &c.**

THE mercury of commerce is generally in a state unfit for experiments in chemistry or physics, in consequence of being mixed with various metals, particularly lead and bismuth. In this state of adulteration its lustre soon tarnishes; it weighs lighter than when pure; leaves a blackish spot when poured on an earthen plate; divides into drops which though roundish are generally more or less compressed, and have an appendage or kind of tail; and on being exposed to the action of the fire immediately abandons the metals with which it was amalgamated.

Distillation is the known and generally-practised method of

* From the forthcoming Memoirs of the Pisa Academy.

purifying

purifying mercury in the state here described. Nevertheless Messrs. Guyton*, Brugnatelli †, Virey ‡, and other chemists, have justly observed, that in this operation a small part of the mixed metals rises with the mercury in the state of vapour; and MM. Klaproth and Wolff § assert, that it is extremely difficult by this means to separate all the bismuth. The iron filings which, according to Nollet, ought to be put into the retort over the mercury to be refined, in order that when the metal rises in vapour it must pass between the particles of iron, assuredly do not contribute to produce a better result; and in fact, no modern author whose works I have been able to consult, recommends the above addition to the process. The continued agitation of this mercury in contact with the air was announced by Priestley as capable of purifying it. If (said he) it is a long time agitated in a stopped decanter of whose capacity it does not occupy above a fourth part, and the air in the decanter be repeatedly renewed by means of bellows, the foreign metal will be converted into oxide, and the mercury become so pure as to resist all proof of extraneous matter by distillation. Nicholson, who said that he had repeated this method with success, recommended it to mathematical-instrument-makers, who had not the necessary apparatus for distillation. But Guyton with some very just observations dissents from this, and is of opinion that the adulterated mercury cannot by such means be liberated from all extraneous matter ||.

In the year 1798, considering that at the temperature of the atmosphere, some acids, as for example the sulphuric, do not dissolve mercury, although they unite with more or less facility to the above-mentioned metals, it occurred to me that it might be practicable to purify adulterated mercury by immersing it in one of these acids and repeatedly changing its surface. The result obtained by this kind of parting having answered my expectations, I have always used the same method on succeeding occasions, and it is well known to many persons, who at my suggestion have put it in practice. Nevertheless I do not pretend that by such a process this mercury becomes perfectly pure, but only in a state fit for use in a great number of experiments. The *most pure* is justly considered by chemists as solely that which is separated from cinnabar by the united action of caloric and iron filings, or that which by the effect of caloric alone is obtained from what is called red precipitate—that is, the red oxide of mercury, by means of nitric acid ¶.

* *Annales de Chimie*, xxv. 79. † *Trattato Elementaire di Chimica Generale*, iii. 136. ‡ *Traité de Pharmacie*, ii. 356. § *Dizionario di Chimica del Sig. Prof.* Moretti, iii. 93, 101. || *Annales de Chimie*, xxv. 77. ¶ Thus silver of cupellation is pure; but the purest is that which is obtained by the decomposition of muriate of silver.

Neither

Neither do I pretend that the method of which I speak is positively new; perhaps it may be known, and probably other artificers for private interest have kept it secret*. But as I have not seen it noticed in any book, I have thought it proper to describe briefly the experiments which I have made on this subject, from which at least it may be more generally known that by means of some acids, the mercury of commerce may be purified sufficiently to serve in a great number of experiments not only in general physics, but also in chemistry†.

I put in different vessels the quantity of three pounds in each; over one I poured a sufficient portion of strong vinegar, and over the other diluted sulphuric acid. By agitating and often shaking the vessels the mercury divided into small globules, and presented more points of contact to the acids; and I observed that they became turbid, and that with reagents they indicated having metallic matter in solution. Continuing the purification and renewing the quantity of acids, on the fourth day I separated the mercury from them, and after washing, drying, and passing it through a hole made with a pin in the bottom of a funnel of writing-paper, I placed it in different vessels. The mercury purified in this manner had a bright surface even after the space of several days.

This result encouraged me to try directly the same process on the adulterated mercury of commerce; and I made the experiment with success. I purified a great deal at different times, preferring however the diluted sulphuric to the acetic acid, because it may be used even still stronger, and it has a more powerful action on the extraneous metals. Such an operation, in which the sulphuric acid may be renewed a greater or lesser number of times, succeeds in a longer or shorter period; but it is not considered as terminated until the mercury divides, and continues for a considerable time in very minute globules, and without altering the acid, which must retain its transparency, and evince no trace of any metallic substance by reagents. In March 1813 I distilled in an earthen retort about seventeen pounds of mercury, which I used for various experiments, and which contained much lead and tin. Although it had abandoned an abundant share of these metals, yet by the pellicle on its surface it was evident that it still contained much amalgam, an effect to which the still had

* In 1806, my friend and colleague Sign. Savi, then Professor of Experimental Philosophy (now Professor of Botany, and the unassuming author of several valuable tracts on the indigenous plants and forest and other trees of Tuscany), observed that a Milanese barometer-maker, purified his mercury by means of a fluid, the nature of which he studiously concealed.

† My assistant in the laboratory annually constructs many barometers and thermometers, and he finds that mercury purified in this manner is of great advantage.

perhaps

perhaps somewhat contributed, from its being so little curved that some particles might escape in the ebullition into the receiver. Not wishing to undertake a new distillation, I placed it in six vessels, and covered the mercury in each about the depth of an inch with diluted sulphuric acid. After five days of repeated and frequent agitation, having washed, dried, and passed it through paper, I found that it left no black stain on a delft plate, that its drops left no appendage or tail, and that it evinced a most brilliant surface, which did not tarnish during the space of several days that I kept it before applying it to use.

But whoever wishes by this process to purify mercury much amalgamated, should use sulphuric acid not diluted, in order to hasten the operation. In fact, having exposed mercury which had been used for what is called the revival of sheet-lead, and for the preparation of plain mirrors, to the action of this acid, in the space of some days it became equal in goodness to that which had undergone the preceding operation. The facts, however, which I have described, demonstrate that the above process is sufficiently useful in purifying mercury adulterated by other metals; and to confirm it, I was induced in May 1815 to resort to the following new experiments.

First. I made by means of heat four amalgams of the following compounds. 1. Two denari* of lead and two of bismuth with two ounces of mercury. 2. Four denari of lead with two ounces of mercury. 3. Four denari of tin with two ounces of mercury. 4. Two denari of lead and two of tin with two ounces of mercury. I then added to each of these six ounces of mercury.

Secondly. I put these amalgams, which had a dense and wrinkled pellicle on their surface, in so many bottles numbered with their respective numbers, and added concentrated sulphuric acid sufficient to cover each amalgam to the depth of two or three lines of a quarter of an inch. Afterwards by repeated agitation and changing the acid several times, I obtained in a greater or less number of days mercury, which I have, and which retained, after being kept many days, the most beautiful brightness. The amalgam No. 2 was the first to yield pure mercury, the last was No. 1.

In this and in other experiments where I used concentrated sulphuric acid, I observed almost instantly around the amalgam a whitish, yellowish, or grayish powder, which always increased, and which I separated at the first washing, by putting into the bottle a new quantity of acid. With agitation the mercury divides into larger or smaller globules; not immediately, nor in a given time, but usually when it is somewhat purified. In the process more or less sulphurous acid is disengaged, and also sul-

* A denaro is the twenty-fourth part of an ounce.

phuretted

phuretted hydrogen gas, which is not only sensible to the smell, but is proved by the test of paper moistened in a solution of acetate of lead; from which it appears that, besides the sulphuric acid being decomposed, the water with which it was united is also at the same time decomposed.

Lastly. By the same means I have separated mercury from the amalgam of mirrors*.

This experiment, which is pleasing in theory and principle, may even serve to *demonstrate in the shortest period of time that, from sulphuric acid, sulphur may be extracted*. In fact, putting in a two-ounce phial, for example, a quarter of an ounce (six depari) of the said amalgam, and adding concentrated sulphuric acid sufficient to cover it from one to two inches deep, and aiding the operation by shaking the bottle, that all the amalgam may mix with the acid, after a few minutes, particularly when the temperature of the atmosphere is not low, a most vivid ebullition takes place, accompanied by copious vapours, by the disengagement of much caloric, of sulphurous acid, of sulphuretted hydrogen gas, and also of sulphur. The last remains attached to the neck of the bottle, and in the greater quantity the less the mouth of the bottle may be. Separating the residue, which is more or less whitish, by washing, the amalgam contained much less tin than at first; and submitting it again many

* Van Engestrom to obtain the mercury from this amalgam proposed to distil it with powdered charcoal or with sulphur. *Ann. de Chimie*, xxvi. 293. [*Note by the Translator.*] Another experimental proof of the purity of the mercury thus refined, is the excellence of the thermometers made with it by the enlightened author's assistant†, with one of which I was presented at Pisa, and which I found exactly corresponding to a very good one made in London. In all the experiments which I have made with these two thermometers, I have not been able to discover, even with a magnifying glass, the least difference in their sensibility. Whether in the open air of the plains, on the Pisa mountains, in the vale of the Arno, the insalubrious furnace (in winter perhaps it may be called basin) of Florence, the heights of Feisole, the Apennines, the Pisa baths which are at the spring 100°, at that of Jove 94, and that of Ceres 93, or the baths of Lucca, which at the spring within the cave are 130°76, at the hotbath 126°,&c.--the rise or fall of the mercury in both instruments was identically the same. The day on which I examined the Lucca baths, when the thermometers were exposed to the direct rays of the sun at the foot of the hill beneath the baths, and on the sheltered bank of the river, they rose in a very few minutes to 116°, and would have risen still higher had they been allowed more time. Both the Lucca and Pisa hot waters, like those of Bath, contain very little extraneous matter, much less than might be inferred from their high temperature and their respective situations near the base of lofty ridges of calcareous mountains: some depositions or incrustations of carbonate of lime appear at their source, but they contain very little gas and no iron. Dr. Franceschi, physician at the Lucca

† In Savoy, Piedmont, and even Tuscany, persons who make barometers and thermometers are wholly unknown.

many times to the same process, after having triturated it, and agitated it with the same acid, it first became fluid, afterwards divided into globules, and finally appeared purified mercury similar to that produced by the preceding experiments. Hence it appears that, by the action of sulphuric acid aided by the mechanical division effected by agitation, the adulterated mercury of commerce, and even that which contains a greater portion of extraneous metals, may be purified in a manner sufficient to serve all the common purposes of experimental philosophy and chemistry. This method does not require continual attention, is not expensive, particularly where the mercury is little altered, and does not expose the operator to any danger.

LVII. *Answer to* W. H. G.'s *Observations on Mr.* Tatum's *Experiments on Vegetation. By Mr.* J. Tatum.

To Mr. Tilloch.

Sir,—Your correspondent, W. H. G., in reply to the paper I communicated to your Magazine of July, accuses me of "unpardonable ignorance for pretending to enlighten one of the most controverted subjects of experimental science, by views and experiments which have been detailed in half a dozen professed treatises, and otherwise promulgated in every possible way."

Lucca baths has published some account of them: but, as usual with writers on baths, his work is more panegyrical and historical than chemical;—a much better account may be expected from his relative Dr. Domen. Pieri, professor of chemistry in the college of Lucca. A very satisfactory and able analysis of the Pisa bath waters may also be expected from the modest and ingenious author of the preceding memoir; who pursues the discovery of facts with unremitting zeal, and leaves the development of crude theories to those self-called chemists whose pen, ink, and paper are more useful to them than acids, retorts, and furnaces. There are still persons who ascribe these hot-baths to the influence of volcanoes; but there is not the smallest trace of any thing like volcanic matter, or even any combustible substance, to be discovered within many miles of them; nothing that, either chemically or geologically speaking, could sanction the belief that they owe their warmth to exhausted subterraneous volcanoes. Vast ridges of mountains surround them, entirely of carbonat of lime or hard and coarse marble, with occasionally veins of felspar, rock crystal, and very rarely traces of tourmaline; but very considerable intersections of these calcareous masses frequently occur, consisting of various combinations of magnesia and lime, forming all the gradations from the hardest to the most friable schist. Gypsum and pyrites, particularly the former, are of rare occurrence, and never in such quantities as would sanction the conjecture that the caloric developed by the decomposition of the latter might contribute to raise the temperature of these springs. It is true that in the valleys adjoining these hot springs inflammable gas, chiefly carburetted hydrogen, abounds; but whether connected as a cause or an effect is not so easy to determine.

From which I infer that he imagines I was unacquainted with any of the six authors alluded to: but in this as well as several other particulars he labours under a mistake. I had consulted "the ill-digested experiments of Dr. Ingenhousz," the excellent and truly valuable volumes of Mr. Ellis, and the still more *recent* publication of Sir Humphry Davy; nor was I quite ignorant that Saussure, Scheele and Sennebier had entered the lists as disputants on this controverted subject. But should your correspondent ask, Then why advance opinions in opposition to the "old story of the purification of the atmosphere by vegetation, which had been treated of by others?" I would answer, that although the opinion is old, it is neither forsaken nor abandoned, nor is it left to sink without a powerful support to rescue it from oblivion: for, notwithstanding W. H. G. marshals the name of that truly excellent philosopher Sir Humphry Davy in the list of the half dozen who have opposed the old story of Dr. Priestley, and who he says have so *long* anticipated my opinions, I have no doubt but that, with all my "unpardonable ignorance," I shall be able to prove that he has been most egregiously mistaken. I will not retort his own language on Mr. W. H. G., but content myself with remarking, that from the diversity of opinions displayed in the above authors, it was not very unnatural that I should entertain a wish to investigate this controverted subject, and particularly as I thought many of their detailed experiments were not unexceptionable. For example; who can approve of the effects of detached leaves while confined under pump-water being brought forward as a proof of the effects carried on by an entire and living vegetable while exposed to air? In my experiments I *endeavoured* to obviate objections of this nature:—how far I have succeded, I shall leave to others to determine. One of the motives which induced me to commit their results to your Magazine, was, that during my last course of lectures I was informed by a gentleman attending the lectures of the Surrey Institution, and who was present at my lecture on vegetable chemistry when I introduced some of *those* experiments, that the then chemical lecturer at that Institution had promised to prove that vegetables improved the atmosphere, but that he had not made good his promise or even attempted it: on which several gentlemen who were present, observing that the experiments militated so materially against the popular opinion, expressed surprise that I did not make them public. Under these circumstances I communicated them to your Magazine.—But to return to your correspondent, who says, "Dr. Priestley was the person with whom the *old story* originated; but that even he seems afterwards to have been aware of the inaccuracy of his conclusions:" for he says in vol. iii. p. 273,

"In

"In general the experiments of this *year* were unfavourable to my former hypothesis; for whether I made the experiments with air injured by respiration, burning of candles, or any other phlogistic process, it did not grow better, but worse; and the longer the plants continued in the air the more phlogisticated it was. I also tried a great variety of plants with no better success." From this one would imagine that W. H. G. thought the Doctor was about to abandon his old opinion: but if he had taken the trouble of turning over one leaf, he would have found the Doctor still firmly maintaining his "*old story*," for he says, "Upon the whole I *still* thought it *probable*, from the experiments of *this year*, that the vegetation of healthy plants, growing in situations natural to them, has a salutary effect on the air in which they grow;—for one clear instance of the melioration of air in these circumstances should weigh against a *hundred cases* in which the air is made worse by it."

Is there here any reason to conclude that the Doctor was "aware of the inaccuracy of his conclusions?" Does not the above prove that, so far from altering his opinion, he was determined to support "his old story" in the strongest possible language?

Your correspondent has seen fit to class Dr. Ingenhousz as well as Sir Humphry Davy among the half dozen authors "who *experimentally contradict* the above opinion." Now, as W. H. G. accuses me of "*unpardonable ignorance*," surely we ought not to expect him to betray any in his criticisms on me. But let us see what were the experimental opinions which Dr. Ingenhousz promulgated in p. 23 of his prefatory remarks. He says, "The discovery of Dr. Priestley, that plants thrive better in foul air than in common and in dephlogisticated air, and that plants have a power of correcting bad air, has thrown a new and important light upon the arrangement of this world. It shows, even to a demonstration, that the vegetable kingdom is subservient to the animal, and *vice versâ*, that the air spoiled and rendered noxious to animals by their breathing it, serves to plants as a kind of nourishment." And at section 16, he says, "In order to put my conjecture to the trial, I placed at eleven o'clock, in a warm sunshine, two jars of an equal size, each containing an equal quantity of sprigs of peppermint in pump-water. In one of these was let up a certain quantity of common air. In the other jar was let up the same quantity of air fouled by respiration: at two o'clock the air of both jars was found much improved; and at four o'clock the common air was still more improved." I shall not make any remarks on the manner in which these experiments were conducted; I merely quote the above to prove that your correspondent has erred (I will not suppose wilfully) in representing Dr. Ingenhousz as "one of the half dozen au-

 thors

thors who *experimentally* contradicts Dr. Priestley." So far from this being the fact, it appears from the above quotations that he is endeavouring by all possible means to support him: indeed, the whole tenor of his work (so far from contradicting Dr. Priestley) is intended to corroborate the same. I will not occupy your pages by remarks on Scheele, Saussure, or Sennebier, but proceed to Mr. Ellis and Sir H. Davy. Your correspondent cannot approve more highly of the opinions entertained in the two volumes of the former of these authors than I do. But I think he *again* labours under a mistake when he represents Mr. Ellis's second volume as the last work on the subject; for my copy bears the date of 1811. But that of Sir H. Davy on agricultural chemistry (which W. H. G. quotes) was published in 1813. Under these circumstances I know not by what means he can consider Mr. Ellis as the last author on the subject. But you will perceive that this is not merely the first, last, or greatest mistake W. H. G. has fallen into: for although Mr. Ellis has ably promulgated the same opinions (drawn from experiments differently conducted to what mine were) which I have since advanced; yet I think it is most unwarrantable and unjustifiable to represent "*Sir H. Davy as being convinced* that Mr. Ellis had not been deceived by his extensive researches;" and also of enrolling Sir H. in the list of half a dozen who had experimentally contradicted Dr. Priestley. In order to prove the error of the first statement, I must refer to Sir Humphry's work above mentioned, p. 195, where Sir Humphry says, "Some persons have supposed that plants exposed in the free atmosphere to the vicissitudes of sun-shine and shade, light and darkness, consume more oxygen than they produce; and that their permanent agency upon air is similar to that of animals; and this opinion is espoused by the writer on the subject I have just quoted (Mr. Ellis), in his ingenious Researches on Vegetation. But all experiments brought in favour of this idea, and *particularly his experiments*, have been conducted under circumstances unfavourable to *accuracy* of results."

So far then was *Sir Humphry* from being convinced that Mr. Ellis was *not* deceived by his experiments, that he actually condemns his experiments as inaccurate. But to prove the error of the second statement of W. H. G.; namely, of *Sir Humphry's* being one of the "half dozen who experimentally opposed Dr. Priestley's opinion," we need only refer to the latter part of the above page, and also to p. 197. In the first of these Sir H. says, "In some of the early experiments of Dr. Priestley, before he was acquainted with the agency of light upon leaves, air that had supported combustion and respiration was found purified by the growth of plants, when they were exposed in it for successive days

days and nights; and his experiments are the *more* unexceptionable, as the plants in many of them grew in their natural states, and the shoots or branches from them only were introduced through water into the confined atmosphere." And the next is Sir Humphry's own experiment, p. 197: "The following experiment I consider as conducted under circumstances more analogous to those existing in nature. A turf four inches square from an irrigated meadow clothed with common meadow grass, meadow fox-tail grass, and vernal meadow grass, was placed in a porcelain dish, which swam on the surface of water impregnated with carbonic acid gas: a vessel of thin flint glass, of the capacity of 230 cubical inches, having a funnel furnished with a stop-cock inserted in the top, was made to cover the grass, and the apparatus was exposed in an open place. A small quantity of water was daily supplied to the grass by the means of the stop-cock. Every day likewise a certain quantity of water was removed by a siphon, and water saturated with carbonic acid gas was supplied in its place, so that it may be presumed that a small quantity of carbonic acid gas was constantly present in the receiver.

"On the 7th of July 1807, the first day of the experiment, the weather was cloudy in the morning, but fine in the afternoon, the thermometer at 67°, the barometer 30.2. At ten on the morning of the 15th, I examined a portion of the gas; it contained less than 1-50th of carbonic acid gas, 100 parts of it exposed to the impregnated solution left only 75 parts;" (100 parts of the air of the garden occasioned a diminution to 79) "so that the air was four per cent. purer than the air of the atmosphere; another similar experiment was made with equally decisive result." p. 199. "These facts confirm the popular opinion" (*the old story*) "that when the leaves of vegetables perform their healthy functions, they tend to *purify the atmosphere*, in the common variations of weather, and changes from light to darkness." Are not these principles or opinions the very same as those promulgated by Dr. Priestley? Aye, and by Dr. Ingenhousz too? Yet W. H. G. represents Sir H. as one of the *experimental opposers* of that very system he is actually advocating. From these repeated misrepresentations on doctrinal points on the part of W. H. G., it is even difficult to believe that he himself knew which side of the question the above authors maintain. We here see that Sir H. did not consider himself as betraying *unpardonable ignorance* by publishing his "Views and Experiments," although they had been detailed by Drs. Priestley and Ingenhousz, many years before, and of course had "anticipated" his views.

Again. He says, "The question *still* remaining is, not whether plants have the power of counteracting the vitiation produced by

by the breathing of animals; but whether they are able, during sunshine, to reconvert into oxygen the carbonic acid they form during darkness and common day-light."

The solution of this question I have attempted, and I hope one day to give a satisfactory answer to it.

And, sir, I presume I may also add that I have attempted a solution of the above question, particularly in the third and fourth experiment, as related in my first paper, in which the plants, inclosed in receivers, were exposed not to air vitiated by the breathing of animals; but to common air, during *darkness, common day-light, and sunshine*; the former of the above for seven successive days, and the latter for there successive days, in both of which experiments it did not appear to *me* that the plants had the *power during sunshine to reconvert into oxygen the carbonic acid they formed during darkness and common day-light.*

From these observations it appears that the subject is yet a controverted one, and that there are celebrated authors of the present day whose opinions are at variance with each other; and I think it is meritorious in any one, amid this conflict of contending opinions, to communicate his researches to the common stock; and most certainly I anxiously wait the coming *day* on which W. H. G. has promised to give to the public the results of his critical experiments. But should he be inclined to make any further remarks on me, and expect an answer, he will please to conclude with his real name and address.

I am, sir, yours &c.

Dorset-street, Oct. 20, 1817. J. TATUM.

P. S. Since I last addressed you, I have found that the small pustules on the ice-plant contain a very considerable portion of muriatic acid; but the smallness of the quantity of the fluid which they contain has as yet prevented me from ascertaining with what it is combined. I am not aware that any one has hitherto noticed it.

LVIII. *Acknowledgements to Mr.* WESTGARTH FORSTER; *further Geological Queries, on the Basaltic Strata of Durham and Northumberland: and Suggestions regarding the Situation of the Granite Patches of the North of England, in its Series of Strata. By* A CORRESPONDENT.

To Mr. Tilloch.

SIR, — I BEG to return my best thanks to Mr. Westgarth Forster, for the answers which he has afforded in your last number, page 216, to my 2d Question, as to the true character and position of

of the "Great Whin Sill," or upper Basaltic Stratum, interposed in the lower part of the Coal-measures or Lead-mine Series of Durham, Northumberland, &c.; and for his promised intention of giving Answers, to others of the Questions which I have taken the liberty of putting, through the medium of your pages, as to the Stratification of these northern parts of the Kingdom, with which I was in a degree aware, that he had a practical and intimate acquaintance. Satisfied of Mr. W. F's kind desire and intention, of fully and completely answering my questions, I shall offer no apology, for troubling him with a few remarks, suggested to me by his last communication, partly in the way of modification of my Queries, in pp. 122 and 252 of your last volume, in consequence of the new light that is now thrown on the subject; with some further Queries, for more clearly ascertaining the facts of the Strata, in the parts alluded to:

1st. I now I think clearly perceive, from Mr. W. F's Answers, and what has been published by Mr. Winch and Mr. Buckland, in the 1st part of vol. iv. of the Geological Transactions, that the Basaltic Stratum, of which Mr. Winch shows so long a range of the northern basset-edge, in his excellent Map in the Geo. Trans. really *underlies all the tract of Country to the South of it, into Yorkshire*, and to the SW and W, into Westmoreland and Cumberland; except, where approaching nearer to the surface, on the tops of Strata-ridges or *bumps*, or where it has been locally lifted by Faults, its covering Strata have been locally stripped off or denudated; viz.

On the Tees River, from near the foot of Crook Beck, or of Trent Beck, (opposite the head of South Tyne River,) down to near Winch-Bridge: again, in the lengthened irregularly oval space, between a point somewhere E or NE of Melmerby on the N, and a point somewhere E or SE of Murton on the S (the precise ascertainment of which points is of very considerable consequence, as intimated in my 2d Query, at top of p. 124), to the eastern verge of which last denudated Tract, Mr. Forster has now traced this upper Basalt at Highcup Nick, and great Rundale Beck: again, in a local throw-up near Tynehead Smelting-house (of which more presently): again, perhaps, at Dufton Fell (see further on): again, near the Town of Stanhope: again, perhaps, near Eggleston on the Tees, (quere any thing more than a Dyke, Geo. Trans. iv. 73)?: and again, perhaps, near Riccargil (Geo. Trans. iv. 108)?; as I suppose, in the southern branch of the Raven Rivulet, near Selah ?, there being no such place or stream as Riccargil, marked in any of my Maps: and again, deep in the sinking at Alstone Moor.

To which list of detached local patches of the upper Basalt Stratum, appearing on the surface, or proved underground by

the Miner's operations, in the Mining district (where its proper places are beneath the surface), I shall be greatly obliged, that Mr. Forster, Mr. Winch, &c. would add in your pages, all other instances that may be in their power; with the depths, thicknesses and dips, in each instance.

From this view of the subject it appears then, that I may now add to my 1st Query in p. 123, the further request to be told, whether the Coal and adjacent Limestone, which have been dug between Melmerby Lane-end and Ardale Water*, belong or match to those Coal and Limestone, stretching from the Coast, between the Coquet and Wensbeck, to Stublic, &c.; the peculiar Organic Remains, and succession of Strata in each case, to be well considered before giving such Answer; and, do not these same Coal and Limestone, match to those of Tindal-Fell?, Query 6 in p. 252.

And for like reasons I now infer, with more confidence than is intimated at bottom of p. 123 of your last volume, that Mr. Winch's basaltic edge, traced from Causeway-Park to Timming or Temon, may be further traceable from thence north-westward, and then north, and north-eastward, perhaps round the western slopes of Bolton-Fell Hill? and Kinkery-hill? to Routledge Burn on English Kershope Farm, 1 m. S of the House (which is on the very Border of Scotland), where I have myself seen this Basaltic Stratum, covered by a Limestone of several yards thick, and perhaps underlaid by another Limestone, dipping very fast to the WNW, into the Liddesdale Trough. At Slaty-ford higher up on the same Burn (which the contrary way runs down to Bailey-head) a Limstone is seen, which perhaps may be the same as here covers the Basalt?: and in Craigy Cleuch, ENE of Kershope-house, the Basalt appears again, forming bold cliffs. I shall feel extremely obliged to any Gentleman, who may be able to trace forwards this edge of the upper Basaltic Stratum, to the north-eastward, towards Carter-fell.

9th. I have heard, that at Lewisburn and Plashets there are Coals;—which way do they dip? and do they underlie the Basalt above mentioned, and belong to the lower division of the Coal-series lapping round the Cheviot mass? see my 1st and 7th Queries in p. 122 and 252; or, do they over-lie it, and belong to the Lead-mine series?

10th. Resuming now, as I intimated I should do, the consideration of the vicinity of Tyne-head Smelting-house, I beg to ask, whether, by "the Back-bone or Great Sulphur Vein,"

* Geo. Trans. iv. 113. The *rearing position* of these Strata, seems unnecessarily to have been ascribed to *dislocation and tilting*; because, such highly inclined positions of Coal-seams, on a great scale, are not very uncommon. See Williams's Min. King. 2d Ed. i. 103.

p. 216, Mr. Forster means the same Dyke and Fault, as is spoken of by Mr. Winch, as "the vein called the Devil's Back-bone," p. 80 of his paper in the Geo. Trans.? If these are one and the same Dyke, in what direction does it range, exactly?, and if the regular Basaltic Stratum is denudated by its side (of which circumstance Mr. W. gives no intimation) what is the extent nearly, of the patch of Basalt which is thus exposed?

11th. If the great *Level* to Dufton-fell Mines, commences in great Rundale Beck, *on* the great Whin Sill (p. 217), and this same Whin, is either *the top* Stratum at those Mines, as Mr. Winch intimates in p. 62 Geo. Trans., or the top Stratum but one (the Tyne-bottom Limestone) as Mr. Forster intimates p. 40 of his Treatise, (quere which is correct?); does not the said *Level*, descendingly cut the series of Strata (either dipping towards the level mouth? or thrown down all at once by a Fault?), so as to enter this mine, and effect its drainage, at some considerable depth below the great Whin Sill?.

In hopes that Mr. Forster and Mr. Winch will spiritedly follow up, what they have so well begun, and that Mr. Fryer will join in their efforts, for elucidating the Stratification of the northern part of England,

I remain,

Your very obedient servant,

October 20, 1817. A Constant Reader.

P. S. I beg to hint to Mr. Professor Buckland and others, who seem, as Geognosts teach, to think that *Granite* wherever it appears *in situ*, must be "either a Dyke, or the projecting back *of a substratum* of this substance (Geo. Trans. iv. 110), that *nothing like a continuous Stratum of Granite has yet been proved to exist in England*, as Mr. Farey long ago asserted; although, mistakenly (Phil. Trans. 1811, and Derby Report, i. 151), he then referred the Granite of Mount Sorrel, &c., *to the Red Marl;* but which Marl he has since shown to be unconformably overlying, there and elsewhere, like the Strata under and surrounding Carlisle:—to such Gentlemen I say, I beg to point out, that there is nothing the least *singular*, in the appearance of patches of *Granite* at Bankey-Close, Hindrigs, &c. on the east of Appleby, but that the same agrees exactly with the positions, of the *granite patches* near Shap Fell, Carrick and Caldbeck Fells (P. M. xlvii. p. 43), near Ravenglass, and again on the other side of the Carlisle Trough, at Criffel Hill; all of these, and others which I could mention elsewhere, occupying the place, or part of it, of *the range of the upper Basaltic Stratum*, which it has been my chief object, in this and several previous communications, to trace out and define: and such Granites, do *not*

*not occupy the central parts of the Slate Tracts**, as *the Geognosy* teaches, we are told. In short, the Granites in these cases, appear as huge nodular or imbedded masses, in this basaltic stratum; which upper Basalt in other instances, as in the north-western slope of Arnton Fell in Liddesdale, and several others of the Basaltic Hills of Scotland, is seen graduating or passing into coarse Slate†, as I conceive is also the case, in the tract on the east of Appleby, of which three very extraordinary *fancy* or Geognostic Sections are given, in plate V. of the Geo. Trans. vol. iv.

I lament that Mr. Winch in p. 207 of your last volume, seems to have declined the task, of attempting to explain, what appears ænigmatical, in the accounts of this tract E of Appleby, which have been published by Mr. Buckland and Mr. Fryer: and I heartily wish, that divesting himself of all undue deference to great *Names*, Mr. W. would in this case, as lately with regard to the pretended *newest* flœtz Trap of another Geognostic Gentleman (p. 123), freely communicate what he knows or can ascertain concerning this tract, and regarding the view of it which I have ventured to offer as above.

LIX. *Geological Observations on Strathearn* ‡.

The wonderful revolutions to which the surface of the globe has been subjected since its primary formation, have of late years claimed the attention of philosophers. Those changes, almost every where apparent, have given rise to new theories no less singular than satisfactory, and have excited a desire in mankind to become acquainted with the causes by which those extraordinary phænomena have been occasioned, and which, in former times, either

* On turning over the pages of your xlth volume, I find Mr. David Mushett aware of the fact, of what certain writers are in the habit of considering as *primitive* masses, not occupying *the central situations*, which on such a supposition, *they ought to be found in*, and in p. 51 says, that on the contrary, in most mountainous districts, these primitive masses (as they have been called) are contained in the superficies occupied by the "great Red" boundary, to the Coal-series; and at the bottom of p. 53 and top of p. 54, Mr. M. more particularly refers to *granite*, amongst the pretended primitive masses above alluded to. Now to me it seems plain, that the "Great Red" of Mr. Mushett, underlying the Limestone, and that Limestone underlying the Coals, of Staffordshire, Shropshire, South-Wales, and Forest Dean, (p. 51), is no other than the same, with the series of Basalts in the South of Scotland and North of England.

† In your xliiid volume, p. 341, I find that Mr. Farey in 1814 suggested this *change, locally*, of the *upper Basalt* or 1st Toadstone, *into Slate*; with reference to the High-peak of Derbyshire and the vicinity of Ingleborough Hill in Yorkshire.

‡ From Blackwood's Edinburgh Monthly Magazine.

either escaped their notice, or appeared so mysterious and inscrutable as to preclude all research.

The human mind cannot now form any conception of that aspect which the surface of the earth originally had assumed, though it cannot be doubted, that from the various agents employed in the mighty operations of Nature, exerted in giving form and stability to our planet, considerable irregularity must all along have diversified it; but those immense masses, which constitute what are called the primary mountains, seem in a great measure to have remained unaltered during the subsequent convulsions that produced the secondary structure, and gave to the universal body its present unequal appearance:—but a smooth and uninterrupted surface was incompatible with those laws which are supposed to have been called into action in the formation of the earth; and though it is not necessary, on the present occasion, to enter into the merits of the contending Volcanic and Neptunian theories, we must still be conscious that many series of facts constantly presented to our view on the exterior, as well as those that have been explored in the bowels of the earth, are consistent with, and may very plausibly be attributed to, the influence of both powers.

For the purpose of exhibiting an object of geology more immediately within the reach of our own observation, we shall confine our remarks to an extraordinary change to which the beautiful and fertile valley of Strathearn has anciently been subjected; and which, though perhaps of less importance to the naturalist than the prodigious altitudes and extensive dales of the Alps and Andes, are still worthy of admiration, as this tract possesses a variety of subjects interesting to the student of nature, and to the lover of her sublime and picturesque beauties.

The great chain of the Grampian mountains, which constitutes the northern, as the Ochil hills do the southern, boundary of this valley, are in many parts composed of primitive matter; but in several places this formation is surmounted by secondary rock of various character and diversity of alternation and position. The portion of those mountains in the vicinity of Lochearn, and what forms the immediate limits of that lake, is not wholly granitic, their exterior being covered with wacke, different species of schistus, lime, and sandstone. Some beds of trap are also visible in its usual linear direction, traversing these rocks without regard to their stratification, and always disposed in vertical walls.

But the most striking features in the district of Strathearn are, the surprising changes that the ground has undergone by the different courses which the river has taken at various periods. These alterations are very evident in travelling along this extensive tract, from the departure of the river out of its parent lake

to

to its confluence with the Tay, a distance of near thirty miles, as the numerous channels by which it has run may be traced with tolerable accuracy.

It appears almost certain, that Lochearn at one time had extended to more than double its present magnitude, having occupied the whole of the flat from its south-eastern extremity to Ochtertyre, covering the great plain on which the village of Comrie, the remains of the Roman camp of Dalginross, the Victoria of Ptolemy, and many farm-houses now stand*. This opinion is strengthened and rendered satisfactory by an examination of the surrounding country, or what originally marked the borders of the lake, where the soil and banks formed by the water are visible, and still retain their first appearance, although for ages submitted to the operations of agriculture. The soil over all this flat is also of a decisive character, being composed of water, gravel, and alluvion, as almost all the stones that have been dug up are round or elliptical, the certain effects of water; and this is particularly the case in the neighbourhood of Ochtertyre, along the road from Crieff to Comrie. On the south side of the valley, near the House of Struan, there is a large concretion of breccia, the composition of which is sand, and stones that have undergone attrition by the action of water, and have been consolidated by the admixture of metallic oxide. This species of rock is not commonly to be met with in the interior of the kingdom, and in no situation but where considerable bodies of water either now are, or have formerly been. On the western shores of Scotland it is frequently seen; but we are not acquainted with its appearance in masses of great magnitude at a distance from the coast, nor in situations of very lofty elevation.

The efflux of Lochearn, in its then extensive form, seems to have been different from the course which the river at present follows in leaving the plain of Dalginross, and appears to have passed from Ochtertyre, whose lakes are the remains of the ancient eastern boundary, along the hollow at the manse of Monivaird, near to which it was joined by the water of Turret. At the present day, the old and perhaps original bed of the river Earn can plainly be traced along the west side of the town of Crieff, where it still intersects two of the streets, sweeping, in a circular direction, the base of the hill on which that town is built, and passing eastward, held its course upwards of 90 feet higher than the present river. Pursuing that direction, it appears to have made several windings until it reached Abercairney, whence

* It has been supposed, by many learned antiquaries, that on this spacious plain was fought the celebrated battle of the Grampians, betwixt the Caledonian and Roman armies; and, certainly, the names of many places in the neighbourhood go far to sanction such a belief.

it

it continued its channel, with little variation from a straight line, nearly due east, running along the tract of the Powaffery river, now a retrograde stream, over the valley where moulder the ruins of the abbey of Inchaffery; and, holding the same line, passed below the House of Balgowan, and the Castle of Methven, until it joined the water of Almond at Pitcairn Green, at that period probably an arm of the sea, which then certainly covered large portions of the flat land along the banks of the Tay near Perth. Over the whole of this ground undoubted proofs of the effects of water are evident, by an examination of the debris collected at different times, which form a variety of strata, and contain boulder stones of many species, brought from the mountains by successive floods and inundations of the river.

But, after the river had ceased to flow by the course which it has thus been supposed primarily to have taken, the valley of Strathearn seems to have undergone other considerable revolutions from the changes of its river.

We have said that Lochearn, according to its original expanse, formed a lake, from its western extremity to the House of Ochtertyre, of twenty miles long, but of irregular breadth. The catastrophe which diminished it to the present size, and gave the river a new direction, does not seem inexplicable. It is the opinion of many profound geologists, that the western mainland of Scotland, with its numerous islands and promontories, were anciently united, forming a compact and undivided continent; but that by tremendous convulsions, produced by general as well as by partial earthquakes, a disjunction of the primary structure was effected, and occasioned that separation of islands from the mainland, and on the mainland, that astonishing irregularity of coast, so indented with arms of the sea, which renders its navigation so intricate, but gives to the mineralogist an ample field of research, and to the painter an admirable display of sublime scenery.—To the cause that has produced such wonderful phænomena do we also attribute the reduction of ancient Lochearn.

The departure of the river from the great level plain of Dalginross, the former bottom of the lake, is through a narrow chasm, the sides of which appear at one time to have been united, as they are composed of the same materials, and were disjoined by some of those convulsions of the earth, which, even of late years, have been so common in that vicinity. This disunion must have been sudden, though from the very remote period at which we may believe it took place, no calamitous consequences as to human life could have happened, as the kingdom was probably not inhabited for many subsequent ages. By the sudden separation of this hill, the north side of which was washed by the lake,

lake, an impetuous and irresistible discharge of water would be the consequence, which, forcing its way through a different tract of country from the former stream, must have carried every opposing substance before it, and speedily have formed a new channel for itself. But this latter course, from passing along a more enlarged plain than formerly, has produced considerable alterations on the face of the country, which is evidently broken by deep hollows that have been washed out by the stream.

The river in the plain near Comrie has taken various channels after the ground was drained by the breaking out of the water that anciently covered it; and when it descends below Crieff, the whole low land is marked by the numerous courses it has pursued at different periods. To trace these windings is not an arduous undertaking; but, excepting in a few instances, a particular description might not be generally interesting. The deep chasms, however, exhibit some objects of mineralogical curiosity, and the steep banks expose a series of alluvial stratification, illustrative of the revolutions to which the soil and surface of mountainous countries are liable.

Having exhausted too much of your time, on a subject of little importance perhaps to your readers, we have only to observe, that in pursuing similar objects of inquiry, sources of rational amusement may be developed, which may ultimately lead to the acquisition of knowledge and the prosecution of useful science, while they must direct the mind to the contemplation of that Power whose wisdom has ordered, and whose omniscience has regulated, the magnificent and wonderful operations of Nature, so constantly under our observation.

Crieff, Aug. 1, 1817. DICALEDON.

LX. *On the component Parts of Light, and the Cause of Colour. By* CHARLES CARPENTER BOMPASS, *Esq.**

IF a beam of solar light be passed through a triangular prism, it is divided into several parts of apparently different kinds. On one side of the expanded sun-beam or spectrum are rays which are invisible, which have been named calorific rays; then are seen various coloured rays, in the order of red, orange, yellow, green, blue, indigo, and violet; and on the other side are some other invisible rays, which have been called chemical rays, being of a different nature from the calorific rays. The rays in the centre of the spectrum, Dr. Herschel has found, have the greatest power in producing vision; which power gradually diminishes towards each side. Neither the calorific nor chemical rays are

* From his Essay on the Nature of Heat, Light, and Electricity.

confined

confined to that part in the spectrum which is visible; each extends from its own side, in quantity gradually lessening, through all the visible rays; so that, as far as they are united, vision is produced; and when they are separated, it ceases. Nor does it appear ever to have been satisfactorily proved, that either the calorific or chemical rays have been entirely separated from each other, while the power of producing vision continued. And no effects are produced by light, except those relating to vision, which may not reasonably be attributed to one or both of the invisible rays. These circumstances would naturally suggest the possibility, that vision may be caused by peculiar combinations of those rays which when separate, or so nearly so as to have the same effect to our senses, are invisible*.

Dr. Herschel's experiments on the subject are by no means conclusive against this hypothesis. He has separated some of the rays from the different coloured divisions of the spectrum; and has found, that the diminution in the power of rendering objects visible, was in a different proportion, from the loss of power to raise the thermometer. But without here inquiring whether the calorific rays are the same as caloric, it is evident, that if the degree of light depend, not only upon the quantity, but also upon the proportionate union of the calorific and chemical rays, the excess of either might be lessened, without reducing the power of producing vision, proportionately with the absolute quantity of the rays abstracted†.

Another

* This opinion was, I believe, first before maintained by Dr. Hunter, in an inaugural dissertation at Edinburgh, in the year 1808; but which it has not been my good fortune to be able to meet with.

† The labour and attention bestowed by Dr. Herschel on the examination of the effect of the different coloured light, on the thermometer and on vision, was very great; and had it been possible to have obtained satisfactory results, they could not have been better earned. But the sources of inaccuracy in such a course of experiments are so numberless, that no dependance can be placed upon them. The careful mention of the mode of experiment has, however, rendered the discrepancies too apparent to mislead.

Thus, it is probable that the colour black, frequently arises from an almost exclusive attraction for the calorific ray; certainly it prevents the transmission of rays in their coloured state. Yet in the first experiments, which were intended to ascertain the effect of the coloured rays of the spectrum upon the thermometer, its bulb was blackened.

In the other experiments, innumerable causes of error are apparent. The temperature of the glass through which the rays were passed is unnoticed; yet perhaps it may be gathered from the experiments, that as that increased, the rays were transmitted with a rapidity equal to that with which they pass through the air alone. The cause of the opacity of glass distinct from its colour is unknown, and incapable probably of being at present ascertained; glass made of the same materials not always vitrifying with the same transparency. Nor, possibly, is the human eye capable of that observation which will enable it correctly to mark a difference so small as

1-10,000dth

Another objection to this hypothesis is, that heat has never been discovered in the light from the moon. But this has been ably shown by Sir Humphry Davy*, and other philosophers, to deserve no weight. The small degree of light which is received from the moon in comparison with that from the sun, as 1 to 300000, perfectly explains how, in the experiments hitherto published on the subject, no heat has been discovered. It must also be remembered, that the lens probably stops a greater proportion of a small quantity of rays of light transmitted through it, than of a larger quantity, its attraction for it being stronger; consequently, it has not an equal power in concentrating rays of light from the moon, as from the sun.

The prismatic colours once separated, are not further divisible by a second prism; and this fact has been urged as a proof of the distinct and homogeneous nature of each of the colorific rays. But the passing of a coloured ray a second time through a prism, does not separate from it the calorific or chemical rays; and therefore, it does not prove that they are unnecessary to vision. For since they do remain united, notwithstanding the degree in which they are usually refracted, it is certainly possible that the cause of their continued union may be the only cause of their continued colour. If it be supposed that they remain in combination from an attraction for the calorific ray, why not suppose an attraction between the calorific and chemical rays? The different colours would then arise from a combination of the calorific and chemical rays taking place in definite proportions, analogous to innumerable chemical attractions. The continued union of the invisible rays, whether there be distinct colorific rays or not, tends to show that there is no repulsion between the particles.

There are on the other hand very great objections to the supposition that the colorific rays are homogeneous. In most cases

1-10,000dth part. For, as a more practised and experienced one than that of Dr. Herschel cannot reasonably be expected, the discrepancies evident upon these experiments may prove its impossibility. Thus, out of 1000 parts, he found that

Coach glass one side rough, stopped	464	parts of heat and	854	of light.
Crown glass ditto	571	..	885	..
Coach glass both sides rough	667	..	932	..
Crown glass ditto	735	..	946	..
All together they stopped	854	..	995	..

But if a calculation be made from the quantity which they stopped separately, it will be found that they ought all together to have stopped 979.8 parts of heat, and 999.938 of light. In another part of the same paper in the Philosophical Transactions, it would appear that the coach glass with one side rough stopped more heat from the taper when alone, than the same coach glass when assisted by crown glass also rough on one side.

* Elements of Chemical Philosophy, vol. i. part i. p. 202.

of

of the division of the ray of light, it would seem from reasoning impossible that it should be so, notwithstanding that the compound coloured ray may be repassed through a prism without decomposition. Thus suppose, for example, a triangular prism to have its side an inch in breadth. Then let a solar ray of light, of the breadth of the side, be passed through the upper angle, and received at the distance at which the spectrum will be two inches broad. Now it is not the whole ray which is at once divided, but every part; and as the degree of refraction depends upon the angle of the prism, the red ray which is refracted at the upper part of the prism, must be so in the same degree as the similar ray at the lower part, and consequently must be parallel with that similar ray. The red ray of the upper part, therefore, must in the spectrum be one inch from the red ray of the lower part, as well as when they first issue from the prism; and the remaining part of the divided solar ray, which passed through the upper part of the prism, will be diffused through the whole upper inch of the spectrum. In the same manner the violet-coloured rays of the solar beam of the upper and lower parts of the prism must be parallel; and, consequently, must be one inch apart, and that of the lower part must fall exactly upon the red ray of the upper part; and the divided ray of the lower part of the prism must be diffused through all the lower inch of the spectrum. Those parts of the solar ray which are between the extremes of the prism, will in like manner be divided, and have their coloured divisions parallel to the similar coloured divisions of the extreme parts; and, consequently, the red ray of the different parts will be diffused through the inch between the upper and lower red ray; and the violet through the inch between the upper and lower extremes of the violet colour; and the intermediate colorific rays of every part, between the extreme colours of that part. In the centre of the whole spectrum, the ray will be compounded of every colorific ray of the spectrum, and there will be no part of the spectrum, except its utmost limits, homogeneous. In this case, for the sake of simplicity, the prism has been supposed one inch, and the spectrum, two inches broad; but, whatever might be the breadth, if the solar ray passed through the prism were of any considerable extent, the effect must be, in its proportion, similar; so that no part of the spectrum except the perfect extremes could be homogeneous. Yet in all these cases, the ray of light seems simply divided into the common prismatic colours; and each of the colours seems as pure, and distinct, as when the solar beam is more narrow; and they are all equally capable of being passed through another prism without decomposition.

In confirmation of this reasoning, the effect of different phos-

phorescent bodies would appear to prove conclusively, that each coloured ray contains the component parts of every other. Some of these bodies, when exposed to light, are capable of absorbing it to a considerable extent, and again emitting it in the dark; and this, for any number of times. The light which each of them emits, is always of the same colour; and this colour it absorbs and emits to whatever coloured ray it may be exposed; so that from every coloured light it is able to obtain light of its own colour. This seems hardly capable of explanation, except upon the supposition that the coloured rays are not homogeneous, but are capable of decomposition by the different attraction of bodies.

That this is possible, is further confirmed by the fact, that light of one colour may be formed by the combination of two other differently coloured rays of light. Thus the blue and yellow rays combined, form green rays. This could not be the case if the green were homogeneous; but if the colour arise from the proportions of the calorific and chemical substances, it would necessarily follow, that if a kind of light containing an excess of one of the ethereal matters should be mingled with another kind of light, containing an excess of the other, the combined light would be of an intermediate kind, and accordant in its nature with that which, from other causes, contained similar proportions.

If the hypothesis before mentioned be just, it should explain the other phenomena of colour; and the degree of ease and correctness with which it does so, will form the surest test of its truth.

Sir Isaac Newton proved, that bodies attracted the whole solar ray; and as the ray is not bound together by attraction of cohesion, they must attract every part, and consequently both the calorific and chemical rays. They do not, however, attract them with the same force. This is evident, from the different chemical influences of the two rays, which must arise from the different degrees of attraction for different substances. But if bodies attract both kinds of ethereal matter, and in different proportions, if they should be placed in an atmosphere containing an excess of both, they will attract, and consequently reflect both, in quantities proportioned to their attractions*. The combination

* This is probably liable to some modifications, from a well-known difference in the energy of the two rays; particularly in those substances, of which the colours seem in some degree to change with the degree of light. The calorific ray is less impeded by opposition to its progress, than the chemical ray. It is less easily reflected, less refrangible, less stopped by a misty atmosphere. It is possible therefore, that from this cause, in some cases, exactly the same proportions which are attracted may not be reflected. This different energy may perhaps be accounted for, by supposing that the calorific

nation of the proportions reflected forms their colours. This is strongly confirmed by the fact, that where no chemical change takes place, the mixture of different coloured substances, such as pigments, invariably produces some intermediate colour; that is, that two substances attracting different proportions, when mixed, always attract proportions compounded of those two.

The absolute visibility of objects, therefore, may be supposed to arise from their attracting the calorific and chemical matter from a distance, which are combined as they are reflected. Their colour depends upon the proportions attracted, and its brightness upon the quantity reflected, which is produced by the force of the attraction, and the polish of the body. The degree of visibility of bodies depends upon the force of the attraction, the polish of the body, and the equality of the proportions of the ethereal fluids.

The colour black may arise from the attraction of either one of the ethereal substances, or from the absorption of all which it may have attracted. It is probable, however, that neither of these cases is ever complete; and that the old opinion, that there is no perfect blackness, is correct. Darkness is the same sensation in the eye as blackness; and so evidently so, that almost all the words in the language applicable to the one, are used to describe the other. Perfect blackness, therefore, would probably render an object as invisible as total darkness. The contrast however, between light and shadow, and whiteness and blackness, frequently makes the apparent effect complete; and in each case, that depth of shade and colour appears perfect, which a more careful examination shows to be widely removed from it: while no known palpable substance is absolutely invisible, except from its transparency; consequently, none can be perfectly black.

The more frequent cause of the common degree of that colour, seems to be the absorption of nearly all the light attracted; and this is sometimes occasioned by mechanical construction. It has been observed, by Thenard, that when a piece of phosphorus of a pale yellow colour is melted in hot water, and then plunged into cold water, it becomes perfectly black; but it recovers its original colour, on being re-melted. Dr. Brewster also, in preparing prisms of realgar, by melting it between two plates of glass, found that this mineral, which has naturally a dark orange colour, became of a darker colour as the heat increased, till at a certain temperature it became perfectly black. It always, however, resumed its original colour upon cooling, even though im-

calorific particles are the larger; and if the force of every equal part be equal, the united power of the parts of the larger particle will be more effectual than the divided action of an equal bulk composed of smaller particles.

 mersed

mersed in cold water*. The colour of charcoal, probably, depends chiefly, or only, upon its mechanical form. The experiments on the great power of acquiring heat possessed by the colour black, would also induce the belief, that it usually arises from the absorption of light.

Bodies of every colour, probably, absorb a part of the ethereal matter attracted by them; and as the reflection of the attracted light is from a mechanical cause, the proportion of the two ethereal substances absorbed, will be the same, or very nearly so, as those attracted. This will produce an expansion of the bodies; which expansion cannot with reason be assigned to one only of the ethereal fluids, if both be absorbed, but must necessarily arise from the whole of the ethereal matter which enters into combination. The ethereal fluid therefore, which causes expansion, must be of different kinds in different bodies. This may be shown by experiments.

If bodies are luminous from the attraction of the eye for light, the colour of the light must depend upon the nature of that attraction; consequently, all bodies which are luminous only from their containing an excess of ethereal matter, as where they are so from temperature, appear of the same colour †. This is liable to some evident modifications. For if by any means, such for example as chemical combinations, a quantity of light is disengaged, the attraction of the eye must be affected by the greater facility of obtaining peculiar proportions. And though light is never so absolutely disengaged, as to be wholly unattracted; yet substances may, by stronger chemical affinities, have the attraction for a portion of the ethereal fluid so weakened, that it may be considered, when under the influence of another attraction, as if absolutely set free. Different substances, it is well known, in their combinations with oxygen, emit light of different colours; and as the ethereal matter in the oxygen must in all cases be of the same nature, the other substances must have emitted different kinds. Those substances, therefore, must have contained different kinds; for they could not have emitted that which they did not contain. And as all the ethereal matter which they contained must have tended to their degree of expansion, we are brought to the same conclusion as before, that caloric, or the ethereal cause of expansion, is not of the same nature in all bodies.

* Journal of Science and the Arts, No. III. and see the observations there.

† The difference of the colour of heated solids as their temperature is raised, probably arises from a cause before alluded to, the different energy of the two rays; whence it happens, that at first as the light is barely able to overcome the difficulties in its reaching the optic nerve, the calorific ray is more prevalent, giving the red tinge, which gradually fades to white, as the eye is able to acquire the proportions naturally attracted by it.

Light

Light produces the same chemical effects which are attributed to caloric; indeed a property always ascribed to light is, that it occasions a rise in the temperature. Some effects, however, are usually esteemed peculiar to the operation of light, as distinct from its action in producing heat. This may easily be explained, by presuming a circumstance very probable to be the fact, which is, that the ethereal fluid commonly afforded to the crucible or retort, may be compounded of proportions different from those contained in the solar beam. This is confirmed by the experiments of Count Rumford, who found that many of the effects usually attributed to the peculiar operation of light as distinct from caloric, may be produced in an oven, probably from the difference in the nature of the heat *.

On this hypothesis, chemical decompositions will take place, either from one of the component parts of the body combining with the ethereal matter with more force than with the other component parts; or from the attraction of the different component parts being for dissimilar proportions of the ethereal fluid, and with an intensity greater than that with which the parts attract each other. And possibly the influence of these combined attractions, which always operate in chemical combinations, decides, in a considerable degree, those specific proportions in which other bodies are particularly disposed to unite; it being evident, that if a fourth substance be added to three others for which it has a chemical affinity, the proportion in which it will be most disposed to combine, cannot be produced by an attraction which it has for one, but must depend upon the proportionate attraction which it has for each, and which they have for each other.

To put, as an example, one of the simplest cases. If two substances, having a chemical affinity for each other, should attract the same proportions of the ethereal fluids, but one should attract them with double the force of the attraction in the other, the influence of the ethereal fluids would be, to induce a tendency in the bases to combine in proportions as one to two. If one particle of one substance should attract the two fluids with forces as four and two, and a particle of the other with forces as six and three, three particles of the first would attract them with forces as twelve and six, and two particles of the other would attract them with the same forces: consequently, the same quantity of the ethereal fluids would attract three particles of one of the bases, with the same force with which it would attract two particles of the other; and, according to the general laws of temperature, which produces an equalization of the acting force of attraction in all bodies, it would tend to cause their combination in the proportions of three and two.

* Philosophical Transactions.

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But bodies generally, or always, attract different proportions of the ethereal fluids; and all chemical attractions, it is probable, diminish in force towards each particle, as the number of those particles increase; while the attraction which other bodies have for the ethereal fluids, at all times operates to regulate the acting force of the attraction in the combining substances. The simple effect above described must always be very materially modified by these circumstances, as well as by the degree of attraction which the bases have for each other; not, however, so as to render particular proportions of no consequence, but, probably, so as to occasion several specific proportions, in which the substances would be disposed to enter into combination with different degrees of intensity.

If, however, the attraction between the bases should be very small in comparison with that which each of them might have for the ethereal fluid combined with itself, it is probable that the only way in which their attractions for each other would act, would be as compound particles, formed of each base and the ethereal fluids in combination with it. And when, from circumstances, this very weak attraction could operate with effect, they would probably unite in every proportion. This probably may be the explanation of the mode of combination of fluids, and of the solution of some solids, which may be mingled and diffused through each other in all quantities.

LXI. *On the pretended Parallel Roads of Glen Roy.* By A CORRESPONDENT.

To Mr. Tilloch.

SIR, — I AM an enthusiastic admirer of geological researches, although a mere novice in the application of its curious and complicated principles to practice in the field; and having lately read, in Vol. IV. of the Geological Transactions, Dr. MacCulloch's very elaborate account of Glen Roy, Glen Glov, and others in the vicinity of Fort William in the Highlands of Scotland; I was anxious to learn without delay, the opinion of a mineral surveyor of some eminence, with whom I am acquainted, as to the sufficiency of the explanation offered of this singular phenomenon by Dr. MacCulloch, whose paper I found he had read; when the reply of my friend was, " The pretended parallel roads of Glen Roy are the *edges of horizontal strata*, and were not occasioned either by the labour of man or the action of the surface of water, as has been supposed." He went on to explain to me, that such tracts of perfectly *level* strata, at any considerable elevation above the present sea, as he believed to exist around Glen Roy, were

were not less rare than the phenomenon of these roads or lines, there, is said to be: but, that *alternations of strata*, inclined in different degrees, so commonly *occasion a change of the inclination of the surfaces* formed by their edges, when considered across or perpendicular to their planes, that it was the most common of geological phenomena; and that mineral surveying, as it had been discovered near a quarter of a century ago, and taught by *Mr. Smith*, the author of the Map of the Strata of England and Wales, &c., most effectively availed itself of this phenomenon, in the investigation of the internal structure of districts, from the form and nature of what appears on their surfaces.

I endeavoured, but without effect, to persuade my friend to make Dr. MacCulloch's paper the subject of a communication to your work, and therein to offer, at length, the reasons for the opinion he had formed as to the cause of the singular parallel and level *lines* on the surfaces of the hills in question. The too common plea of want of leisure was brought forward as his excuse; and thus I have been induced to trouble you, in order to throw out the above suggestion, while this paper of Dr. MacCulloch may be under the consideration, or fresh in the recollection, of your geological readers, some of whom, I hope, will enter zealously on the further investigation of the subject, and promote the putting to the test the explanation thus offered, by cutting trenches down through the alluvia (as miners formerly were wont to do, in search of mineral veins), and sufficiently into the strata, to ascertain their nature and positions, right across the lines or pretended roads, in several places.

I am yours, &c.

October 4, 1817. A Geological Novice.

LXII. *On Cosmogony.* *By* H. S. Boyd, *Esq.*

To Mr. Tilloch.

Sir, — An eminent mineralogist lately put into my hands Kerr's Translation of Cuvier's Essay on the Theory of the Earth. Although it lay quite out of the road of my ordinary studies; yet as it came recommended by such an authority, I was induced to peruse it with attention. To this work Professor Jameson has prefixed a preface, in which he intimates that the Mosaic history is remarkably confirmed in four particulars: 1. The order in which the different animals were created: 2. The occurrence of the deluge: 3. The precise period of its occurrence: 4 The recent origin of the human race. Yet in other respects the

French philosopher seems at variance with the Jewish historian. It appears not only from him, but also from Jameson and from Werner himself, that the creation occupied a period of some thousand years; that fishes were formed long before land animals, and land animals long before man. Jameson thus endeavours to reconcile these apparent discrepances. He supposes, with Bishop Horsley, that when our globe was originally formed it may have revolved on its axis much slower than it does now; and consequently that each day of the creation may have been a period of a thousand years, or even a longer term. Now if we admit that this is a complete solution of the difficulty, we shall be granting little; for there are other difficulties which appear far greater, and at which Jameson has not even glanced. I will state those which have struck me most.

1. It is most evident, from the account of Moses, that all the animals which were created on the fifth and sixth day were alive on the seventh and at a subsequent period. But according to Cuvier, fishes without number perished before the sixth day commenced, and beasts innumerable perished before the sixth day was closed.

2. It is clear, from the words of Scripture, that all living things were made for the use of man. But Cuvier informs us, that a multitude of fishes, and most probably of beasts, also perished before the formation of man.

3. If only some individuals of a species had perished, or even if some species of a genus had become extinct, it might have been said that, as the genus was preserved, the purpose of the Creator was accomplished. But it appears from Cuvier that whole genera of fishes became extinct, and that new ones succeeded them before the formation of land-quadrupeds, and that whole genera of land-quadrupeds became extinct and were succeeded by others. Hence it is apparent that whole tribes, both of fishes and of beasts, were created without any reference to man.

4. Moses teaches us, that the earth was adorned with its beauteous garniture of trees and plants on the third day, but that no living thing knew the luxury of existence until the fifth. But Cuvier instructs us, that fishes, and crocodiles, and serpents, lived and died long before the earth was fitted for the support of land animals. Professor Jameson is most decided on this point. His great work on mineralogy was published nine years ago. In the third volume he asserted that fishes appear to have existed before land-plants were produced.

5. According to Moses, the reptiles were formed at the same time with the beasts; but Cuvier tells us, that serpents and oviparous animals in general were contemporaneous with the fishes.

6. From the account which Moses has given us of the deluge, it

it is clear that the highest mountains were completely covered. Cuvier however says, that when the primitive mountains had once appeared above the ocean, they were never again submerged. Jameson, in the volume already cited, asserts the same thing.

7. All orthodox Christians are agreed in this point: That if there had been no sin, there would have been no suffering; that suffering of every kind is the effect of sin; that Adam was constituted the head and representative of the whole creation; and consequently that all the animals participated in the consequences of his disobedience. But in this respect the Christian doctrine is overturned, and, I may say, annihilated, by the system of geologists. According to them, whole races of carnivorous animals inhabited both the sea and the dry land before the creation of man; consequently the brute creation must have been in a state of pain and suffering before Adam fell. Cuvier, indeed, allows, that while the face of the earth was covered with successive races of land animals, the human race may have been existing in some narrow region of the globe. But this will not remove the difficulty; for Jameson and Cuvier both assure us that the sea was peopled with fishes before the dry land was capable of being inhabited.

8. Nothing whatever in the Scripture is more clearly taught, than that the whole human race has descended from one father and one mother. Cuvier has an interesting chapter, in which he inquires what are the greatest changes which can be produced in the race of animals by time or climate, or any other cause. He decides, that the greatest possible difference which can be produced, is not so great as the difference between one species and another species of the same genus. I think it must be acknowledged that an European and a Chinese differ as much from one another as two species of a horse, or a dog, or any other animal. Now as no reason can be assigned why time or climate should operate greater changes on men than on animals, it follows that the European and the Chinese could scarcely have been of the same species. But the difference between an European and a Negro is far greater and more decided. It is therefore scarcely possible, according to Cuvier, that they could have sprung originally from the same parents.

9. At the time of the deluge the earth was covered with the same race of men and the same kinds of animals as those which now exist; they must therefore have been overwhelmed together. But Cuvier tells us, that although the strata of the earth abound with fossil bones, there is scarcely a single instance of a bone being found which belongs to an animal of an existing species; and he maintains that there is no instance whatever of a human bone being found in a fossil state. He maintains at the same time, that no reason can be given why bones of men should

not

not have been preserved, as well as those of land animals. It necessarily follows, that when the earth was inundated by the last deluge it was not occupied by the present races of men and beasts. Cuvier and Jameson will perhaps endeavour to remove this difficulty, by saying that at the last deluge the ocean changed its bed; that what is now the sea was formerly the dry land; and consequently that the inhabitants of the antediluvian world are buried beneath the great deep. But I would ask, Is this Scriptural? Is it not contrary to the general tenor of the Book of Genesis, and particularly to the description which is given of the garden of Eden?

I could point out one or two more difficulties; but I think that those already stated are sufficient to exercise the ingenuity of even the ablest man that England or Scotland can produce. I shall certainly feel the highest gratification, if any man of science will reconcile one-half, or even one-quarter, of the contradictions which I have enumerated.

I remain, sir, your obedient servant,

Margate, Nov. 4, 1817. H. S. Boyd.

LXIII. *New Quadratic Theorem.* By Joseph Reade, M.D.

To Mr. Tilloch.

Sir, — Should the following method of extracting the square root from compound quantities meet your approval, you will please to insert it in your Magazine. The Editor's remarks will oblige Joseph Reade, M.D.

Rule.

1st. Arrange the compound quantity according to the dimensions of some letter, and set the root of the first term in the quotient underneath. 2dly, Multiply the root by 2, and divide the second term by the product, placing the quotient under the second term. 3dly, Multiply the last quotient by 2, and divide the third term by the product, placing the quotient under the third term. 4thly, Square the last quotient, and by the product divide the last term. If nothing remain, the square number is measured by the square root thus found. Like signs give plus, unlike signs minus.

We have inserted Dr. Reade's rule, as it may be of use to some mathematicians; but it is evident from the consideration of any compound term $a+b+c$ that is squared. The square being always equal to the sum of the squares of each single sum added to

to twice the products of those terms taken two and two. All the instances accompanying the rule are too long for insertion, but we have selected one.'

$$a^4 - 4a^3b + 8ab^3 + 4b^4$$
$$a^2 - 2ab - 2b^2 - 0. \text{ square root.}$$

Here the root of the first term a^4 is a^2, which we place in the quotient; secondly, we multiply this root by 2, giving for a product $2a^2$, by which we divide the second term $-4a^3b$; the quotient is $-2ab$, which we place under the second term; thirdly, $-2ab \times 2 = 4ab$ and $+8ab^3 \div -4ab$ gives $-2b^2$ for the third term; lastly, $-2b^2$ squared $= +4b^4$, which subtracted from the last term leaves nothing. Therefore the square root is $a^2 - 2ab - 2b^2 - 0$.

LXIV. *Notices respecting New Books.*

A Letter to Professor STEWART, *On the Objects of General Terms, and on the Axiomatical Laws of Vision. By* J. FEARN, *Esq.* pp. 32.

Of the two subjects treated in this letter, the author states the latter, on the Axiomatical Laws of Vision, to be that to which he is more particularly desirous of attracting public attention. The matter of the Laws of Vision he presents as exhibiting what he considers to be a *mathematical analysis* of the *constituents* or *cause* of VISIBLE FIGURE; and as falling properly enough within our range of philosophical duty, to contribute to the general investigation of a view certainly somewhat novel of an interesting branch of physics, we shall extract at length that part of the letter which relates to it.

"ON THE AXIOMATICAL LAWS OF VISION.

Preface.

"The most proper preface to the following subject, on the present occasion, appears to be that of introducing the fact asserted by Proclus [alluded to in the address prefixed to this publication]. In stating this fact, however, it may be of no small consequence to note, very particularly, that although its truth must attest the truth of the laws of vision, (which is my reason for bringing it forward here,) yet if the fact could be actually disproved, this could not at all affect these laws, since they do not depend upon, but include, the fact asserted by Proclus. Yet, nevertheless, I must add, that I believe myself to have distinctly proved the fact in question; which, it is to be remarked, is *not proved* by Proclus, but only asserted by him.

"In

"In Mr. Taylor's translation of the Commentaries of Proclus on the first book of Euclid's Elements, vol. i. page 125, is this passage: 'We should admit the followers of Apollonius, who say, that we obtain the notion of a line when we are ordered to measure the lengths alone, either of ways or walls; for then we do not subjoin either breadth or bulk, but only make one distance the object of our consideration. But a line may become the object of our sensation, if we behold the divisions of lucid places from those which are dark, or survey the moon when dichotomized; for this medium has no distance with respect to latitude, but is endued with longitude, which is extended together with the light and shadow.'

"The perspicuity of the description of this fact is highly conclusive and valuable. But I cannot avoid remarking, how strange it appears that any philosopher who had adverted to this fact in the *particular* instances of 'the divisions of *lucid places* from those which *are dark*,' should not have intuitively discerned that the principle is *general, universal, and sole:* which it must be, since *light* and contiguous *shadow* produce in us *two sensations of colours* with *a line between* them, just as is and *must* be done by *any other* two colours whatever.—His not discerning the *universality* of the fact was the only thing that could have kept Proclus from advancing on, to discern the four laws of vision and their axiomatical nature, together with their direct consequences.

"OF THE EXTERNAL CAUSE OF VISION.

"1. *Distant bodies* are *not*, by any medium, the *generic* cause of vision; since sensations of *colours*, accompanied by *figures*, are as constantly, and as variously, excited by experiments of pressure upon the eye, and by other bodily affections, as they are by *light* reflected from *distant objects*.

"This general fact being duly recognised, ascertains of itself the independence of vision upon external *distant* bodies, and removes a very great and most pernicious stumbling block, which has strangely been suffered to remain an obstacle to all advancement, although uniform experience has long demanded its expulsion from the subject.

"2. When the optic organ is stimulated, either by light, by sensible pressure, by certain bodily diseases, or by any other such impulse, the mind undergoes a set of *sensations* called colours. Such are those beautiful phantoms that appear to us when we look at a rainbow, or a landscape. These phænomena seem to adhere to external distant objects, like a skin cast over them: but there is no fact upon which philosophers are more unanimous, than that they are nothing but our own *sensations*. It is therefore

therefore here assumed, *as a first principle*, by universal consent, that *phantoms of colour* are but '*a species of thought.*'

"3. With this only settled principle, it has ever been one of the greatest problems in philosophy to discover the *nature* and *place* of those outlines, that are seen as it were surrounding the phantoms of colours, and to which we give the appellation of *visible figure.*—There now exist only two opinions concerning this matter: perceived figures are either the real identical forms of external and distant bodies; or, they are actually the forms of our own sensations, which, if so, do not *show*, but only *indicate*, some *unknown external* cause. The highest authorities of the last century have divided upon this point: and the literary public, impressed by the untoward character of the schism, appear to consider all proof, or foundation on the subject, as a desideratum utterly hopeless. Such is the discouraging introduction to the following principles.

"OF THE POSSIBLE CASES OF VISION.

"All the possible cases, or accidents, of primary vision fall under *four* general facts, or laws.

"Each of these four laws is also an *axiom:* its truth does not depend upon the laws of *Nature*, but on the law of *thought;* since, the moment it is apprehended, we discern that its contrary is impossible. This forms the most striking and important character of the phænomena of vision.

"Two of the laws of vision are *unformative*, either of any figure, or of any element of figure.

"The other two laws are *formative*, either of some figure, or of some element of figure.

"*First Law.—Unformative.*

"*Prop.* No one uniform sensation of colour can ever be accompanied by a perception of any visible figure, any line, or any point.

"*Inst.* If the eye traverse the unclouded heaven, or if it skim the surface of the sea, we shall undergo a uniform *sensation of one colour;* and here it is self-evidently impossible we should ever perceive any visible figure, any line, or any point, so long as the sight keep within the field of this one colour.

"It is plainly as impossible to conceive a visible line, without calling up some *second* colour, as it is to conceive a boundary to an infinite surface: for, any colour we perceive, must be absolutely without end, if it be not terminated by our view of some *second* colour.

"*Second Law.—Formative.*

"*Prop.* When any two unblended sensations of colours are felt

felt at the same time, they *must meet* by their nearest edges, and this *meeting* we must perceive as *a line*.

"*Inst.* If the eye traverse either the firmament, or the ocean, until it arrive at, and take in, *any second* colour; the evidence we have for this fact can be no other than our being conscious *where one sensation* of colour *ends*, because the *other begins*. This *meeting* of the two sensations of colours, is *a line* of *contrast and of contiguity* in our view: and a perceived line, therefore, is purely nothing but *a thought of discrimination*, which we make between two of our own sensations. At the same time it is plain, that we can no more avoid perceiving the *contrast*, and the *extended direction of this contrast*, than we can avoid being conscious of the two different sensations of colours which form this contrast.

"*Third Law.—Formative.*

"*Prop.* When any two unblended sensations of colours are felt at the same time, and are so disposed as that one of them embraces or surrounds the other, we must perceive a line of junction, which is where the embraced sensation meets that which embraces it. Such a line must return into itself, and thus is formed every complete figure that the visive faculty can strictly apprehend.

"*Inst.* When we look at the moon, surrounded by the azure sky, we suffer a *sensation of silver white*, embraced by *a sensation of azure*, and the line perceived between these two sensations returns circularly into itself; which people take for the circle of the moon.

"It must be an obvious truth (although it is overlooked by Proclus) that, *whatever* be the *hues or tints* of the two sensations employed, there can be but *one universal principle* that gives any perception of a *line between* them; and this principle is *a perception of contrast*.

"*Fourth Law.—Unformative.*

"*Prop.* When any two sensations of colours are felt at once, and are blended or softened at their nearest edges, they never can be perceived as forming any *line* between them, not, even, if their distant parts be of the most opposite colours.

"*Inst.* Let any surface be conceived to be black all round its edge, and white in its centre, and let the two colours run gradually into each other: no line can ever be perceived from looking within the field of this surface.

"Innumerable other instances of this fact may be had, as when we look at waving corn, or shot silks, spheres, mirrors, or drinking-glasses.

"This fourth law strikingly illustrates the other three; because herein

herein we suffer *two* sensations of colours with *a negation of all figure, or line,* between them; and here, therefore, we are, by a *new result,* more vividly (though not more certainly) convinced that it is *not colour, but contrast* that is the *creative principle of* any perceived *visible figure, or line.*

"To conclude. Visible figure is a *positive* thing to *our view,* but only a *relative* thing in regard to the *two sensations of colours* which combine to give it being: it is nothing but the *local or co-extended relation* of one sensation to the other.—To say, therefore, that we perceive visible figure, is to say that we perceive the co-local or co-extended relation which one sensation of colour bears to another one, felt at the same time.

"It follows, upon the highest *kind* of evidence, that visible *figure* is nothing but *a creature of the percipient,—a thought of the mind,*—yet, a thought resulting from the action of *some external cause* stimulating our visive constitution.

"THE LAWS OF VISION ARE MATHEMATICAL AXIOMS.

"The four general facts of vision are herein called only laws, because their subjects are, in the first place, sensible or natural phenomena. But it must be insisted upon that they possess a far higher title, in being *mathematical axioms.*

"What renders this consideration most important, is, that even could it be proved that visible lines are not mathematical as to the property of being void of breadth, this (as has been already remarked) would not hinder the laws of vision from being mathematical *axioms* in *the class* of their evidence, the *self-evident necessity* of their truth.

"Physical laws (it is agreed) are not necessary, in our conception: they rule what *is,* but may not rule what *shall be:* light may fail to excite sensations of colours in the human mind; and sensations of colours may, for aught we know, be excited in minds without eyes: all this is conceivably possible. But, to conceive any *one* sensation of colour with a boundary or *line* to it; or, to conceive any *two* sensations of colours at once *without a line* between them, is an impossibility of the very *same class,* as to conceive an infinite surface with a limit, or *two* contiguous mathematical surfaces without the line that makes them *two.*

"Now this *perceived necessity* of the laws of vision, is a paramount test that a visible line is not an external thing; because, it is not merely an object of *sense,* but is also an object of *intuition;*—it is not merely a thing that *now is,* but a thing that *ever must be,* if its *co-efficients* exist. Every *external* object is a thing that may not be at any future time; and, while it exists, we know not its co-efficients: but, we absolutely know the co-efficients of a visible line, by the same process of *rationality,* and

and to the same *perfection*, that we know that the co-efficients of *any idea of relation* must be *some two things, between which* the mind perceives this relation. Here I must refer to the small tract I published some time ago upon '*necessary connection*;' in which my object is to show that we absolutely know the *co-efficiency of all our ideas of relation;* and in which I suppose the thing is rigidly proved. Now *sensations of colours are ideas;* and I repeat it here, that we have the same degree of cognisance of *their relations* (one to another) that we have of the relations of equal, double, or half, between any two mathematical quantities; that is, we perceive the *necessity of the relation* so long as the *two subjects* exist, and we intuitively perceive that the relation cannot exist unless its two subjects exist.

"What a change in the assumptions of mathematics, to find, that its conclusions are *not* limited to hypothetical or conditional truth, but embrace also *facts,—concrete facts!* What an enlargement of the field of demonstrable subjects!

"VISIBLE LINES ARE VOID OF BREADTH.

"This general fact (it is always to be remembered) is wholly subordinate to the laws of vision, being included in those laws but not necessary to their truth. At the same time, however, it is a fact rigidly demonstrable.

"A mathematical line (of the *schools*) is demonstrated to be void of breadth, in consequence of its being defined to be 'the common boundary of two contiguous surfaces.' Now, if one of the two surfaces be supposed blue, and the other one yellow, it is plain the mathematical line of contiguity, and the line of contrast of the two colours, is *one same line;* and since it has no breadth as the common boundary between the two surfaces, it can have no breadth as the common boundary between the two sensations of colours.

"To attempt to invalidate this upon the ground of the imperfection of sense, would only prove that the person who undertakes it does not apprehend *all* the terms of the subject. The subject is *a line* that *we see:* and (without any appeal to the suffrage of Proclus) we may safely maintain that we *don't see* what we *don't see*. The imperfection of sense only makes us *not see* breadth, in some instances where breadth really is before us, and where a magnifying power makes it evident: but the imperfection of sense cannot make us see breadth when it makes us *not see* it. In rigid truth, therefore, the *imperfection* of the organic process of sense causes the *perfection* of the mathematical line we see; for the organ will not convey a report of breadth to the *sentient*, in some cases wherein the *external* object that we look at really has some minute breadth.

"A visible

"A visible line cannot be of any one colour; because it is proved (by the first law) that no one colour ever can have a line. If then a visible line have any colour, it must be a part of each of two contiguous colours: but this would show a *double line* to every object, which we know to be a result utterly contradicted by the fact.—Moreover, if any such double or two lines be supposed, it is plain that each one is but a rim of its own *surface;* and what is *surface* cannot be *line;* neither can two contiguous sensations of colours *appear* to us as forming a *line,* until we mark the *place* where *both colours cease to be,* by reason of their coming in *contact.*

"It is true that we see instances enough of breadth in what are called softened lines, or where two colours blend: but none of these are visible lines; they are all visible *surfaces,* and they must be stript of the appellation of *lines,* in an inquiry like the present.—Visible lines are all those lines which are void of breadth to the *naked eye,* and which can further attest that they are breadthless to the naked eye, by showing no breadth when subjected to a *magnifying power.*—Such lines are raised in our sentient by our looking at the letters of good printing, as divided from the white field of the paper; and such, too, are seen from looking at most other objects.

"It is here an obvious truth, that a visible line which shows no breadth under a magnifying power, can have no breadth to the *naked eye.* It is therefore vain to try to overturn the fact, even if we could by the strongest power produce any evidence of breadth: for it must still remain, that the *natural eye* of man enables him to see *no lines,* but lines that are *void of breadth in his apprehension of them.*

"Finally: But if, in the face of experiment and of common sense, any person choose to assert that *a visible line has invisible breadth;* then, (I repeat it here) this absurd contradiction in terms, if suffered to stand for an objection, could be of no concern to the *laws of vision;* for these laws must still be *axioms,* and *a visible line* must still be nothing but *a line of contrast between two sensations:* and the *contrast line* must still, and for ever, be where the *sensations are* which form it, which is *in the mind itself.*

"*Hereupon,* (urged by the moment of the evidence, and by the infinite magnitude of the consequence,) I make the appeal, in this one question,—Will it (against the *four axioms of vision*) be ever affirmed, that *visible figures* are the *distant things of an external world?* or, Will it be ever affirmed that visible figure is not a phenomenon of the mind?—This is an appeal that *cannot die.*"

LXV. *Intelligence and Miscellaneous Articles.*

SAFETY-LAMPS.

To Mr. Tilloch.

SIR,—YOUR last number containing a particular account of the proceedings at an entertainment, when a superb service of plate was presented to Sir H. Davy, by a number of the coal-owners possessed of mines in this part of the kingdom, as a partial remuneration for the discovery of a safety-lamp; may I request you to give publicity to the liberality of another part of the spirited body of men in rewarding Mr. George Stephenson, who still disputes the priority of inventing a safety-lamp with his more opulent and scientific opponent?

A description of this lamp, as it was exhibited to the Literary and Philosophical Society of this place, in December 1815, will be found in your Magazine for January 1816.

"At a meeting held at the Assembly Rooms, November 1, 1817, for the purpose of remunerating Mr. G. Stephenson for the valuable service he had rendered to mankind by the invention of his safety-lamp, which is calculated for the preservation of human life in situations formerly of the greatest danger:

"*Resolved*, That it is the opinion of this meeting, that Mr. G. Stephenson havtng discovered the fact that explosion of hydrogen gas will not pass through tubes and apertures of small dimensions, and having been the first to apply that principle in the construction of a safety-lamp, is entitled to a public reward."

In consequence of the resolution, between six and seven hundred pounds was immediately subscribed, and it is thought the amount of the gratuity to be presented to Mr. G. Stephenson will not fall short of a thousand guineas. Such rewards do equal honour to the donors and receivers. But as few benefits are conferred on mankind unalloyed by some evil, such I fear is also likely to be the result from the introduction of safety-lamps into the coal-mines; for, what effect can be expected, as far as relates to health and strength, from the use of a lamp which will consign a vast number of workmen to breathe an atmosphere surcharged with carburetted hydrogen? Accustomed from infancy to alternate heat and cold, and compelled to work in contaminated air, the pitmen are far from robust or long-lived; but an invention which will facilitate the opening of old mines for the sake of the coal left in them, but which would not repay the expense of being worked by the light of steel mills, must ultimately prove a curse rather than a blessing to this laborious and valuable class of miners.

Newcastle, Nov. 8, 1817. H.

P. S. When ether is poured upon a lighted lamp covered by a wire-sieve, a lambent blue flame may be observed playing above the wire-gauze; but the flame is innoxious, as it will not set fire even to ether.

I should be glad to hear from yourself, or any of your correspondents, the cause of this phænomenon. H.

Though I have decidedly declared, in former Numbers, that as cool an examination as I could give to the evidence, had afforded me complete conviction that Mr. Stephenson was not the original inventor of the safety-lamp. I have complied with the wish of my correspondent, in giving the foregoing a place. It ought to be preserved as a matter of fact, connected with the history of the sciences. For the same reason I subjoin the following: A. T.

"*Resolutions of a Meeting held for considering the Facts relating to the Discovery of the Lamp of Safety.*

" Soho-square, Nov. 20.

"An advertisement having been inserted in the Newcastle Courant for Saturday, November 8, 1817, purporting to contain the resolutions of 'A meeting held for the purpose of remunerating Mr. George Stephenson for the valuable service he has rendered to mankind by the invention of his safety-lamp, which is calculated for the preservation of human life, in situations formerly of the greatest danger," and asserting,

" 'That it is the opinion of this meeting, that Mr. George Stephenson, having discovered the fact, that the explosion of hydrogen gas will not pass through tubes and apertures of small dimensions, and having been the first to apply that principle in the construction of a safet-lamp, is entitled to a public reward;'

" We have considered the evidence produced in various publications by Mr. Stephenson and his friends, in support of his claims; and having examined his lamps, and inquired into their effects in explosive mixtures, are clearly of opinion,

" 1. That Mr. Stephenson is not the author of the discovery of the fact, that an explosion of inflammable gas will not pass through tubes and apertures of small dimensions.

" 2. That Mr. G. Stephenson was not the first to apply that principle to the construction of a safety-lamp, none of the lamps which he made in the year 1815 having been safe; and there being no evidence even of their having been made upon that principle.

" 3. That Sir Humphry Davy not only discovered, independently of all others, and without any knowledge of the unpublished experiments of the late Mr. Tennant on flame, the principle of the non-communication of explosions through small apertures, but that

that he has also the sole merit of having first applied it to the very important purpose of a safety-lamp, which has evidently been imitated in the latest lamps of Mr. George Stephenson.

"JOSEPH BANKS, P.R.S.
"WILLIAM THOMAS BRANDE.
"CHARLES HATCHETT.
"WILLIAM HYDE WOLLASTON.
"THOMAS YOUNG."

STEAM ENGINES IN CORNWALL.

The following were the respective quantities of water lifted one foot high with one bushel of coals by thirty-three engines, reported by Messrs. Leans' in the month of October.

	Pounds of water.	*Load per square inch in cylinder.*
25 common engines averaged	21,502,796	various.
Woolf's at Wheal Vor ..	31,690,248	15·5 lib.
Ditto Wh. Abraham ..	42,639,545	16·8
Ditto ditto	26,851,707	4·3
Ditto Wh. Unity ..	36,450,897	13·1
Dalcoath engine	44,192,139	11·2
Wheal Abraham ditto ..	38,399,332	10·9
United Mines ditto	29,903,937	18·1
Wheal Chance ditto ..	32,320,346	13·1

ECONOMY OF FUEL.

Sir,—The approach of winter has suggested the propriety of making generally known the following composition, as likely to produce much public benefit, if adopted by the middle and lower classes of society. You will at once understand the principle on which they will answer as fuel, and of course admit this into your valuable journal.

A mixture of sand, clay, and coal-dust or charcoal, or saw-dust, made with water into a moderately stiff compound, are all the materials required, and these may be obtained almost anywhere. The following proportions may be assayed until another more preferable may be ascertained by experiment.

Coal, charcoal, or saw-dust, or the whole mixed together	1	parts.
Sand of any kind	$2\frac{1}{2}$	
Marl or clay	$1\frac{1}{2}$	

These parts may be pecks, bushels, or baskets, or any other measure at hand. The mass to be made into balls of a convenient size, moderately dried, and the work is accomplished.

They will not answer to light a fire with; but the fire once brought to nearly a white heat, these balls will support it, be very durable, produce a heat incomparably more intense than common

common fuel of any kind, and increase the value of the ashes as a manure. The mud swept up in the streets of London, and other paved towns, will answer admirably in lieu of marl or clay, or with a little clay to give it adhesion, as such mud must unavoidably contain a considerable quantity of iron.

It may be observed, for the advantage of those unacquainted with chemistry, that sand is principally an oxide, that carbon attracts oxygen from almost every known substance at elevated temperatures; and oxygen being the cause of combustion, every substance containing it, which can be decomposed, becomes a supporter: these balls therefore will prove of material consideration as a cheap fuel, and will, I judge by estimation, if judiciously managed, produce a saving of one half the quantity of coals used in general. To thousands this must be of valuable consideration.

When the fire is at a sufficient temperature, it will be of still further advantage to sprinkle it occasionally with water; this will afford additional oxygen and produce increased effect: for this purpose a small watering-pot may become a part of the apparatus usually attached to a common grate. The balls also will produce a greater effect moderately damp, than when perfectly dry, and when formed they should not be too much compressed.

I remain, sir,

Your obedient servant,

Hampshire, Nov. 3, 1817. A SUBSCRIBER.

P.S. Balls of a similar composition have been used in Wales from a very remote period.

IRON BRIDGES ON THE PRINCIPLE OF TENACITY.

The following is an explanatory statement, which has been put in circulation, of the principle of tenacity, on which the iron bridges projected by Mr. Dodd, engineer, over the Thames at Hammersmith and Rotherhithe, are designed to be executed.

In the construction of the newly invented iron bridges, on what is termed the principle of tenacity, the objects are, to form and adjust their several parts with a particular view to that important quality of the metal, which disposes it, on being stretched, not merely to resist and keep its hold, but to appear to draw or pull in a direction opposite to that in which the force that sets upon it is applied.

In the construction of other iron bridges the metal is employed like any common hard and bulky substance that is capable of having its pieces connected together; and the several pieces of it are so arranged to rest and press against each other, as if they possessed no other property than their solidity, extension, and weight. In the Southwark bridge, for instance, we see the plates

of iron that compose the arch, cast on a similar plan, arranged in a like order, and depending in the same way upon one another, as the blocks of stone in the arches of Waterloo bridge, and requiring, in consequence of that arrangement, a corresponding bulk and strength in the piers and abutments, not only to bear the perpendicular pressure or gravity of the materials, but to afford an adequate resistance to what is termed the lateral pressure, the pressure of the sides of the arch, or bridge, upon the bases on which they rest. In the construction, however, of such bridges as are proposed to be erected at Hammersmith and Rotherhithe, the iron is made use of so, as that its property of tension should be most effectively and advantageously employed, and the pieces of which the structures are composed, are so adjusted with a view to the mutual dependence of the parts and the independence of the whole, as to diminish the necessity of bulk, without injury to the strength of the fabric; and to promote a proportional lightness in its appearance and effect, at the same time that it almost annihilates the occasion of the lateral pressure. An illustration of the manner in which the weight or pressure operates in reference to such an arch, will enable the reader to perceive the way in which these important objects are attained.

Let the action of an archer's bow be considered, when the upper side of the arch is pressed by the hand, while its ends or points are resting on a table. The force applied upon the bow would produce a spread, which, in the case of a bridge, would be termed its lateral pressure, and which in that case would require a corresponding strength and resistance in the building of the abutments or piers. If the cord, however, be attached to the bow, and the same force as before be applied to press it, the cord would seem to pull and counteract the spread to which the bow would be disposed, and prevent any lateral pressure being experienced beyond its points. In the structure of an arch, if formed as a bow of iron, or in that of a bridge composed of a series of such arches or bows, the like result must be produced, if every arc be furnished with its proper chord of iron, and that chord be, as care should be taken that it should be, of adequate strength. A familiar and accurate idea of such a figure may be conceived, from recollecting that of the brass segment which usually composes part of a case of mathematical instruments. An iron structure of that form, if constituted so as to be made an arch of a bridge, would not on any scale require abutments to resist its pressure, or the weight that might be laid upon it. It would rest at its points upon the upright standards that would be provided to support and raise it above the water, and would press or act upon them only in a perpendicular direction, and in a way that could most easily and œconomically be resisted.

In

In this manner, without entering into a detail that might be perplexing if not illustrated by visible figures, some notion it is apprehended may be formed, of the shape and structure of an arch of such a bridge as is constructed on the principle of tenacity; and of the way in which it is supported and elevated. The same principle is resorted to in respect to the form and arrangement of the several other parts of the structure, wherever it is admissible, by giving to the iron pieces the shape of ribs, and connecting them so as to constitute as much as possible, an independent body that may rest upon perpendicular standards, which are to possess sufficient strength, but to be divested of extravagant bulk. By this construction, the least practicable degree of impediment is presented to the passage of the waters and the navigation of the river; and the greatest œconomy may be promoted in the expenses of materials and labour, and of course of time and money. In the article of iron, one half the quantity it is said, may be saved, that would be requisite to complete a bridge of the same dimensions on the ordinary construction.

Mr. Dodd, the engineer, the inventor of the system, has it seems estimated the expense of the proposed Hammersmith bridge, which will be 600 feet over the river Thames, at 50,000*l.* and that of the designed gigantic structure, the East London, or bridge of Trafalgar at Rotherhithe, though its chord will be 3,400 feet, and its altitude, to allow ships to sail beneath it, will probably be 110 feet above the tide at high water, he reckons will not exceed the sum of 300,000*l.* The latter structure will consist of three arches, of 320 feet each over the water, and eight others, of more than 300 feet each on the average, over the land on either side.

A NON-DESCRIPT FISH.

Captain Mudge, one of the gentlemen employed in the Trigonometrical Survey, has stated, that a few days before he left Shetland, he had received a letter from a gentleman of large property there, informing him, that a fish of very singular appearance had been taken off the island of Unst, where Captain Mudge had been stationed with M. Biot. The fish was to have been sent to Captain Mudge, but it did not arrive in time, and therefore he knew it only from the description given of it by his correspondent, which was very minute and particular. It was of the flat species, about four feet long, and was most amply provided with fins; but its distinguishing peculiarities were two *antennæ* or feelers, about eight or ten inches long, standing erect from the head, each crowned with a fine tuft resembling a flower; whilst on the under part, near the breast, were two hands exactly resembling the human hand, except that they were palmated or webbed.

Captain

Captain Mudge not having time to stay, left instructions with M. Biot, who remained behind for the purpose of contemplating the *aurora borealis*, to have the fish preserved in spirits and sent up to London. We may therefore hope to obtain an opportunity of communicating a more detailed account of this very singular fish, which does not appear to have been described by any writer on Ichthyology.

ANOTHER MAMMOTH FOUND.

Dr. Mitchill, of New-York, in a letter to Dr. Clinton, dated Chester, 27th May 1817, published in a New-York paper, announces the discovery of the remains of a mammoth on the preceding day in the town of Goshen, Orange county, within sixty miles of New-York, in a meadow belonging to a Mr. Yelverton. "The soil," says Dr. M. "is a black vegetable mould, of an inflammable nature, and in reality a good kind of turf. It abounds with pine knots and trunks, and was about thirty years ago covered with a grove of white pine-trees. The depth below the surface, where the bones lie, does not exceed six feet. There is reason to believe the whole osseous parts are here, as they can be felt by exploring-rods in various directions round the spot. It may be expected, that with due exertion an entire skeleton can be procured, surpassing every thing of the sort that the world has seen.

"The region extending from Rochester along the Walkill to this place, is full of organic relics. The fossils indicate the former dominion of the ocean; and many of them appertained to creatures not now known to be alive. The dimensions of the parts as given me by Drs. Seely and Townsend are as follow;

"Length of the tooth, 6 inches. Breadth of the same, 3½ inches. Circumference of the lower jaw, including the tooth it contains, 26 inches. Length of the jaw, making allowance for some detrition, 35 inches. Breadth of the articulating surface of the lower extremity of the humerus, 12 inches. Breadth of the outer condyle of the same, 7 inches. Breadth of the inner condyle of the same, 5 inches. Depth from the antĕrior to the posterior part of this articulating surface, 10 inches. Length of the cavity of the os cranion, 7 inches. Breadth of the same, 5½ inches. Depth of the same, 2½ inches. Length of the ulna, 32 inches. Circumference of the upper articulating surface of the ulna, 32¾ inches. Circumference of the articulating surface of the lower extremity of the humerus, 35 inches."

DE LUC, THE GEOLOGIST.

Died on the 7th inst. at his house in Windsor, after a painful and lingering illness, which he endured with exemplary fortitude, John

John Andrew De Luc, F.R.S., aged 90. That celebrated and indefatigable geologist has committed the result of his laborious and multifarious researches, unremittingly prosecuted for upwards of fifty years, to numerous works which place him on a level with the most distinguished philosophers of this enlightened age. Having visited almost every part of Europe, in order to enlarge his knowledge and increase his collection of facts by personal observations of geological phænomena, Mr. De Luc has thereby been enabled to demonstrate the comparatively small antiquity of our continents, and the difficulty of carrying back their origin to a period more remote than that which the Mosaic chronology has assigned to the flood. It may also be observed, that Mr. De Luc has not only extended the limits of geology, and established fundamental points in that science, but has been a highly successful experimentalist in various branches of natural philosophy intimately connected with it, and in which he has made very valuable discoveries. Those concerning the mode of action of the Galvanic pile are particularly interesting: he has ascertained that, in Volta's pile, the chemical effects can be separated from the electrical; and these last led that ingenious philosopher to construct a new meteorological instrument, very desirable for acquiring a knowledge of atmospherical phænomena, and which he called the Electric Column. It is well known that Mr. De Luc was a strenuous opponent of the new chemical theory known by the name of Lavoisier's. He has shown in his two "Memoirs" on that theory, prefixed to his *Introduction à la Physique Terrestre par les Fluides expansibles*, that meteorological phænomena strongly militate against it; and, in general, that the hypothesis of the composition of water (the fundamental point in the theory) has maintained itself only by numerous other hypotheses which are in contradiction with known facts. Mr. De Luc's theories on evaporation, on the dew, on the formation of the clouds, on rain, &c., are grounded upon the most accurate experiments and patient observation of the respective phænomena.

Mr. De Luc was not less amiable as a man than he was eminent as a philosopher. To the powers of an understanding of the first order he united the most endearing qualities of the heart. The warmth of his feelings, and the habitual gentleness and urbanity of his manners, were admirably calculated to procure to him friends, and to retain them when gained. In the varied relations of life, as husband, father, master, friend, he exhibited the most edifying model of the social virtues. From the situation he held as reader to the Queen, Mr. De Luc had for many years daily access to Her Majesty; and that his faithful services were justly appreciated, was rendered evident from the flattering testimonies

testimonies of attachment and regard with which he was at all times, and more especially during his last illness, honoured by his Royal Mistress.

Mr. De Luc was a member of several academies and universities on the Continent, and maintained a correspondence with the most eminent naturalists and philosophers during the greatest portion of the last century. Many of his writings lie dispersed through various literary and scientific journals, foreign and domestic: among others, the Philosophical Transactions, the Journal des Sçavans, the Monthly Review, the British Critic, the Philosophical Magazine, and the Monthly Magazine. The following works may be mentioned as the most important of Mr. De Luc's publications:—Lettres sur l'Histoire de la Terre et de l'Homme, 5 vols. 8vo, 1779; Recherches sur les Modifications de l'Atmosphère, contenant l'Histoire Critique du Baromètre et du Thermomètre, 1784; Idées sur la Météorologie, 2 vols. in 3, 1786; Lettres sur l'Education Religieuse de l'Enfance, 1799; Lettres sur l'Histoire Physique de la Terre, adressées au Professeur Blumenbach; Lettres sur le Christianisme, 1801; Précis sur la Philosophie de Bacon, 2 vols. 1802; Introduction à la Physique Terrestre par les Fluides expansibles, 2 vols. 1803; Traité Elémentaire sur le Fluide Electrico-galvanique, 2 vols. 1804; An Elementary Treatise on Geology, 1809; Geological Travels in the North of Europe and in England, 3 vols. 1810; Geological Travels in some Parts of France, Switzerland, and Germany, 1803.

LIST OF PATENTS FOR NEW INVENTIONS.

To John James Alexander MacCarthy, of Milbank-street, Westminster, Middlesex, for his road or way for passage across rivers, creeks, and waters, and from shore to shore thereof, without stoppage or impediment to the constant navigation thereof, and across ravines, fissures, clefts, and chasms; and a new method of constructing arches and apertures for the running and flowing of water through the same, or under bridges, to be used and applied in the construction of the before-mentioned road or way or otherwise.--Dated 28th July, 1817.--6 months allowed for specification.

To Jephtha Avery Wilkinson, late of New-York in the United States of America, but now residing in Covent Garden, for certain improvements in the application of machinery for the purpose of manufacturing of weavers' reeds by water and other power. —6 months.—23d August.

To George Medhurst, of Denmark-street, Saint Giles, Middlesex, for his arrangement of implements to form certain apparatus which he denominates The hydraulic balance,' applicable to mechanical and hydraulic purposes.—28th Aug.—6 months.

To

To James Mason Champness and Henry Binks, both of Cheshunt-street, in the county of Hertford, for certain improvements on axletrees of carriages of various descriptions.—28th Aug.—2 months.

To Joseph Manton, of Davies-street, Berkeley-square, for certain improvements in locks for fire-arms.—26th Sept.—6 months.

To John Dale, of White Lion-street, Pentonville, Middlesex; for his application of a certain material hitherto unused for that purpose to the making of rollers or cylinders of various description. —3d Oct.—2 months.

ASTRONOMICAL PHÆNOMENA, DECEMBER 1817.

D. H. M.		D. H. M.	
1. .	♀ ν^1 ♎ * 13′ S.	12. 6. 4	☽ ε ♑
2. 6 7	☽ ν ♍	12. .	♀ 625 Mayer * 9′ S.
2. .	♀ ν^2 ♎ * 25′ S.	14. .	♀ β ♏ * 5′ S.
3. 9.47	☽ γ ♍	17.14.46	☽ ο ♓
3.23.22	☽ θ ♍	20. .	☽ in apogee
5. 2.10	☽ κ ♍	21.13.25	☽ ♂
5. .	♀ ζ^2 ♎ * 22′ N.	21.15.19	☉ enters ♑
5. .	♀ 603 Mayer * 11′ N.	22.11.22	I } of 125 ♉ * 9 S. of
5.19.14	☽ α ♎	22.12.29	E } ☽ Cent.
6 15.30	☽ κ ♎	24.14.42	☽ υ ♊
7. .	☽ in perigee	25. 6. 0	☽ ψ ♋
8.18.20	☽ ☿	27.12.12	☽ η ♌
8. .	♂ 98k ♉ * 22′ S.	29.12. 9	☽ ν ♍
9.10.53	☽ φ ♐	30.14.42	I } of γ ♍ * 5 N. of ☽
9.14.27	☽ σ ♐	30.15. 0	E } Cent.
. ; 8	☽ τ ♐	31. 6.34	☽ θ ♍

Meteorological Journal kept at Walthamstow, Essex, from September 15 *to November* 15, 1817.

[Usually between the Hours of Seven and Nine A.M. and the Thermometer (a second time) between *Noon* and Two P.M.]

Date.	Therm.	Barom.	Wind.
September.			
15	61 66	29·98	NE—SE.— Hazy; and very damp; cloudy day; cloudy.
16	63 66	30·00	SE.—Hot, damp, and cloudy; cloudy, and windy day; sunshine after 2 P.M.; dark night.
17	53 69	30·00	NE.—Hazy morning; fine hot sunny day; dark night. Moon first quarter.
18	56 60	29·77	N—NE.—Gray; rain early; slight showers; rainy.

September

September			
19	56 63	29·87	SE—W.—Clear, and *cirrostratus* and *cumuli*; gleams of sun; moon and star-light; slight *aurora borealis*.
20	50 62	29·98	W.—Sun and *white* dew, and clear sky; *cirrostratus* and sun; very dark; then sunshine again; cloudy.
21	55 64	30·00	NW—W.—Gray morning; fine day; *cumuli*; perfectly clear; moon- and star-light.
22	42 61	29·98	N.—Sun and *white* dew; hot sunshine; after 2 P.M. gray and windy; cloudy but *light*.
23	50 63	29·97	NW. — Gray morning; clear; *cumuli* and gleams of sun; fine day; *cumuli*.
24	51 66	29·98	N—S.—Hazy; fine sunny day; mottled *cirrocumuli*; moon and stars.
25	59 63	29·60	SE—N.—Cloudy; wind and showers; fine day; cloudy and windy. Full moon.
26	55 59	29·22	SE—SW.—Showery, and *cirrostratus;* very showery day, and sun between the showers; clear moon and star-light.
27	52 56	29·43	SE.—Gray morning; showery after 11 A.M. till 4 P.M.; moon, stars, and *cirrostratus*.
28	48 57	29·76	SW—W.—Gray and windy, and *cirrostratus;* sun and *cirrus;* very fine day; clear moon- and star-light.
29	41 57	30·00	SW.—Hazy; fine sunny day; moon visible, but not the stars.
30	46 54	30·00	N.—Gray morn; a slight shower early; sunny fine day; 7 P.M. star-light; 9, moon in a *corona;* no stars visible.
October			
1	44 57	29·87	N.—Gray morning; fine day; bright star-light.
2	34 47	30·00	N.—White dew; sun and hazy; fine day; sun and wind; clear red sunset; star-light.
3	29 51	30·00	S—N.—Sun and white frost and wind; fine day; star-light.
4	39 43	30·20	NE.—Clear sunshine; fine day; clear star-light.
5	40 53	30·22	E.—Clear above; *stratus* low NW.; sunny morn; cloudy; some drops of rain at 1 P.M.; sun again; star-light.
6	35 54	30·22	NE—E.—Clear, and *cumuli*; very fine day; star-light; *aurora borealis* 11 P.M.
7	45 56	30·10	NE—E.— Clear; *cumuli*, and wind; fine day; star-light.

October

8	39 53	30·22	E.— Gray; *cirrostratus;* fine day; *cirrus;* wind and sun; cloudy, and dark after 3 P.M.; star-light; very beautiful *aurora borealis* at $11\frac{1}{2}$ P.M.
9	41 51	30·00	Clear and *cumuli;* very fine day; cloudy; slight *aurora borealis.*
10	40 58	29·90	N—N by E.—Fine morning; clear and *cumuli;* gray day, but some sun; dark night; *aurora borealis.* New moon.
11	42 52	29·90	N—N by E.—Fine morn; windy; a star early; sun and slight showers; very bright star-light.
12	43 53	30·00	W—N by E.—Fine morn; showers, sun and wind; bright star-light.
13	38 51	30·10	W—E.—Fine morn; *cumuli,* and slight showers and wind; bright star-light.
14	37 52	30·22	N.—Hazy and sun; fine day; sun and *cumuli;* dark.
15	45 48	30·00	N by W.—*Cirrostratus;* showers; star-light.
16	42 43	29·88	N.—Showers and wind; very rainy till about 2 P.M.; fine afterwards; rainy evening.
17	40 49	30·00	E—NW—N.—Fine; wind and *cirrostratus;* showery; star-light. Moon first quarter.
18	42 44	29·98	N.—Fine; windy; showery day; cloudy.
19	43 47	29·88	N—NE.—Cloudy; gray cold day; some drops of rain; cloudy.
20	42 42	29·88	NE.—Cloudy; showery; cloudy.
21	41 51	29·88	N—S.—Hazy; fine day; cloudy, but light.
22	41 49	29·88	S—N.—Hazy, and *cirrostratus;* gray day; moon- and star-light; bright.
23	43 49	29·88	N—NE.—Clear, and *cirrostratus;* rain; fine afternoon; light, but cloudy.
24	42 42	29·88	NE.—Damp, and *cirrostratus;* showery day; cloudy.
25	40 51	29·86	NE—SE.—Hazy; gray day; cloudy. Full moon.
26	43 50	29·76	SE.—Gray morn; clear, and *cirrus;* and sun, and wind; fine day; moon- and star-light.
27	35 50	29·75	S.—Foggy morn; fine day; rain.
28	35 50	29·44	SE.—Hazy low; *cirrus* and clear; high clouds and wind; rain; moon, stars, and *cumuli.*

October

October			
29	39	29·45	SE.—Hazy low; clear and *cirrostratus* high; very fine day; bright moon- and star-light.
30	47 56	29·34	SE—SW—W.—Rain till about 2 P.M.; fine after 2 P.M.; moon in a *corona*; stars, and *cirrostratus.*
31	46 53	29·55	SE—NW—W.—Rain; rainy till after 2 P.M.; 4 P.M. clear; windy, and some *cumuli*; clear moon- and star-light.
November			
1	33 47	30·22	SW.—White frost, and sun; very fine day; star-light.
2	41 51	30·20	SE.—Hazy; cloudy day; dark night. Moon last quarter.
3	50	30·20	S.—Hazy; very damp and cloudy day; dark and damp.
4	57 55	30·10	SE. — Hazy; *cirrostratus* and sun; dark night.
5	47 56	30·10	S by E.—Foggy; leaves fall very fast; very fine day; star-light.
6	51 56	29·90	S by E.—Hazy; cloudy; rain and wind.
7	53 58	29·87	SE.—Clear and *cirrostratus*; *very* fine day; rain.
8	51 53	29·44	SE.—Clear and *cirrostratus*; a shower at 1 P.M.; fine day; windy; star-light.
9	46 51	29·61	S—W.—Hazy; sun and wind; very fine day; dark night. Full moon.
10	46 54	29·97	S.—Hazy; clear above; gray day; some rain; dark night.
11	47 57	29·87	E—SE.—Fine sun-rise, *cirrus* and clear; rainy after 10 A.M. till after 2 P.M.; star-light.
12	46 56	29·77	S—SE.—Clear and *cirrostratus*; fine day, but damp; dark.
13	41 50	29·77	S—SW.—Clear sunrise; some *stratus* low; fine day; star-light.
14	52 53	29·55	Clear and *cirrostratus*; hazy low; very rainy after 9 A.M.
15	55 55	29·32	SE—NW.—Rainy; showers and some sun and wind; star-light. Moon first quarter.

Nov. 15. Ranunculus Flammula, Geranium Pyrenaicum, and Geranium sanguineum shooting forth *fresh* buds, and many plants still in full flower at Walthamstow; a single swallow seen at Clapton the week before the last; very few butterflies, wasps, &c. have been seen during the summer; but in October *many* small flies made their appearance in windows.

METEORO-

METEOROLOGICAL JOURNAL KEPT AT BOSTON, LINCOLNSHIRE.

[The time of observation, unless otherwise stated, is at 1 P.M.]

1817.	Age of the Moon	Thermometer.	Barometer.	State of the Weather and Modification of the Clouds.
	DAYS.			
Oct. 15	5	49°	30·14	Rain—heavy at night.
16	6	50·5	30·17	Fine
17	7	47·5	30·27	Rain
18	8	48·5	30·10	Cloudy
19	9	51·	30·15	Ditto
20	10	49·5	30·11	Rain
21	11	53·	30· .	Cloudy
22	12	50·	30·01	Ditto
23	13	50·	30·13	Fine
24	14	48·	30·06	Cloudy
25	full	48·	29·95	Ditto
26	16	50·	29·85	Ditto
27	17	47·	29·66	Rain—heavy at night—rime frost
28	18	40·	29·50	Ditto—heavy P.M.
29	19	45·	29·59	Fine
30	20	52·	29·25	Rain
31	21	40·	29.80	Ditto—heavy A.M.
Nov. 1	22	48·	30·41	Fine
2	23	48·	30·19	Cloudy
3	24	57·	30·34	Fine
4	25	51·	30·17	Cloudy
5	26	50·	30·04	Ditto
6	27	54·	30·05	Foggy
7	28	58·	29·81	Ditto
8	29	54·5	29·55	Stormy
9	new	51·	29·79	Cloudy—fine P.M.
10	1	53·	30·07	Very fine
11	2	50·5	29·81	Rain
12	3	53·	29·79	Cloudy
13	4	49·	29·88	Fine

METEORO-

METEOROLOGICAL TABLE,

BY MR. CARY, OF THE STRAND,

For November 1817.

Days of Month.	Thermometer. 8 o'Clock, Morning.	Thermometer. Noon.	Thermometer. 11 o'Clock, Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
Oct. 27	40	50	42	29.45	17	Fair
28	38	50	42	·35	0	Rain
29	40	47	38	·37	10	Cloudy
30	50	57	47	·20	0	Stormy
31	46	51	38	·52	0	Rain
Nov. 1	38	52	44	30·30	14	Fair
2	46	55	55	·12	12	Cloudy
3	54	54	50	·07	4	Cloudy
4	52	55	48	·02	8	Fair
5	49	57	50	29·87	16	Fair
6	54	58	52	·78	21	Fair
7	54	57	50	·56	22	Fair
8	53	55	50	·40	10	Stormy
9	52	52	49	·69	17	Fair
10	48	55	50	·96	22	Fair
11	50	54	50	·64	0	Rain
12	50	55	52	·79	10	Fair
13	47	50	50	·72	21	Fair
14	53	55	55	·42	0	Rain
15	55	54	50	·46	0	Showery
16	50	55	52	·94	10	Fair
17	54	59	52	30·20	6	Cloudy
18	53	56	47	·32	10	Cloudy
19	40	50	40	·40	14	Fair
20	38	50	48	·30	5	Fair
21	45	50	47	29·86	17	Cloudy
22	42	46	45	30·14	16	Fair
23	45	47	46	.01	10	Cloudy
24	46	50	40	29·72	11	Cloudy
25	35	40	41	·91	14	Fair
26	40	52	46	30·01	6	Cloudy

N.B. The Barometer's height is taken at one o'clock.

LXVI. *On a volatile concrete Oil existing in the Nut-galls of the Oak. By Dr.* Jos. Branchi, *Professor of Chemistry in the Imperial and Royal University of Pisa. Contained in a Letter from Dr.* Branchi *to Sig.* R. Gerbi, *Professor of Theoretical Physics.*

When I spoke to you some time ago of a volatile concrete oil which I had obtained from oak-galls by the same means with which other volatile oils in general are extracted from aromatic vegetables, I promised to give you a more extensive description of it. I now fulfil my promise in this letter. In the beginning of 1814, having distilled a few ounces of oak-galls broken and steeped in a sufficient quantity of water, I observed swimming on the odorous and turbid fluid which had passed into the receiver some drops of an oil, which in a short time became solid. Various occupations, especially those of my professional chair, and a series of experiments which I had commenced on the chemical properties of colouring matters, hitherto prevented me from investigating this fact. But having repeated the distillation of galls, I obtained, as before, some drops of oil sensibly yellow, swimming on the surface of a whitish odorous and saporific liquid, which in the space of some hours became sufficiently clear, by the precipitation of a whitish substance in flakes, heavier than the liquid itself. On filtrating the fluid through blotting-paper, these thin semitransparent and shining flakes remained, and were the same drops of oil which had taken the character of a white odorous opaque unctuous matter, and of a consistence nearly similar to what is called butter of cocoa. This white matter, it appears, differs from the above-mentioned thin flakes by having less specific gravity, and by its aspect, which is not shining; but both have the property of volatile oil—both have the following characters:

1st. They have a bitter caustic taste, and the same odour as galls when pulverized.

2d. They are slightly soluble in water, which imbibes their smell and taste.

3d. They readily and copiously dissolve in alcohol. The solution which results is rendered turbid by a small quantity of water, but in more or less time it reassumes its transparency by the addition of a proportionate quantity of water.

4th. They unite to fixed oil, as that of olives; and to the volatile oils, as turpentine and lavender.

5th. Exposed on a piece of blotting paper (or even writing paper) to the action of a slight degree of heat, they liquefy and anoint the paper so as to render it diaphanous. Continuing

however the heat, they rise in a visible odorous vapour, and the paper becomes opaque without retaining any trace of unctuosity. Moreover, this oily spot entirely disappears from the paper by the action of the solar rays, and even by the mere temperature of the atmosphere. By the latter mode the spot did not vanish till after twenty-eight days, during which the thermometer of Reaumur did not indicate a temperature above 14°.

6th. Liquefied and absorbed by a cotton thread, they inflame when brought into contact with the flame of a candle.

7th. Spread and pressed on paper coloured with turnsole, the paper reddens very sensibly.

Lastly. The solution of sulphat of iron neither makes them become violet nor black.

These properties, however, sufficiently prove that this substance is a volatile concrete oil, and consequently cannot be confounded with gallic acid. Nor does the circumstance of its reddening the paper containing tincture of turnsole furnish any objection, as M. Vogel asserts that he has found no volatile oil, however recent, which wants the property of changing to red this tincture *.

The liquid from which this oil was separated by filtration, had, like what is called aromatic water, the same taste and smell as the oil itself. It did not change to violet or black the solution of sulphate of iron,—hence, did not contain gallic acid; and it gave a slight tinge of red to the blue of turnsole. This reddening was owing perhaps to the acetic acid, which is found free in galls, and passes over with the water in the distillation, as Bouillon Lagrange has shown †.

The galls from which I extracted the above-mentioned volatile oil are small, heavy, of a reddish-yellow or orange colour, more or less tending to dark or to blackish, wrinkled externally with some protuberances, and not spongy internally. The next object was to know if the same oil existed in the nut-galls of the oak in general; and with this view I procured galls of different qualities which are distinguished by the following appellations: *Italian* or *Tuscan galls*, *Istria galls*, *yellowish* and *black*, *Aleppo* or *Smyrna galls*.

Wishing that the experiments about to be performed should be comparative, I distilled separately six ounces of the above-mentioned kinds of galls with eight ounces of water ‡, in a glass retort placed almost superficially on a sand-bath, the mouth of which touched the bottom of a small receiver which I had united

* *Journal de Physique*, lxxx. 256. † *Annales de Chimie*, lx. 173-5.

‡ The same quantity and quality of galls do not absorb an equal dose of water, so that in the above eight ounces there was more or less superabundant.

to

to the neck of the retort with a moistened bladder; and tied it, after having put a slender thermometer for security. The receiver I kept always cool with wet cloths, and I stopped the distillation when about two ounces of the liquid had passed over, that is, as soon as the galls became apparently dry.

The Italian galls (*galli nostrali*) are generally larger than those already described, almost entirely round, as the constriction which forms the point of union with the stem or leaf is very short; they are light, not covered with tubercles or excrescences, more or less smooth, spongy internally, and of a yellowish colour, more or less deep; they yielded an odorous, saporific, and whitish liquid*, without the drops of oil. Preserving this liquid in a bottle with a ground-glass stopper, after several hours some small white flakes were precipitated, which were heavier than the liquid itself, and on their surface were seen two small particles equally white. The liquid being then filtered through paper, I observed that the paper by a slight heat became in some parts oily and diaphanous, and that by a greater heat these same parts resumed their opacity, while an odorous vapour ascended from them.

The *Istria galls*, which did not perfectly resemble those with which I made the first experiment, furnished an odorous, saporific, very opaque and white fluid with drops of oil on its surface. It is to be observed, that some small drops of oil also appeared in the neck of the retort. This fluid after reposing threw down many heavy particles, and in two or three days became limpid. By filtration I separated about $1\frac{1}{4}$ gr. of concrete volatile oil, partly in the state of drops, and partly in that of brilliant flakes, which had all the above characters.

The *black Istria galls* are small, more or less wrinkled, naturally blackish, somewhat compact internally, and similar to those which, according to Klaproth and Wolff (Dictionary of Chemistry), come from Aleppo and Smyrna. The odorous and saporific fluid obtained by distillation from them was less white and opaque than the preceding; it had on its surface a greater number of drops of oil, which appeared by the numerous oily particles on the neck of the retort, and on settling deposited a few white particles. These particles with the oil weighed $1\frac{3}{4}$ gr., and had all the properties of the concrete volatile oil already described.

Lastly, the *Aleppo* and *Smyrna galls* are of various sizes and colours, as whitish, and inclining to yellow, red, green, and brown;

* This odour, which is owing to the volatile oil, becomes more sensible if we make a decoction of the galls in the matrass. When the liquor has boiled a little, the odour diminishes in consequence of having evaporated the greater part of this oil.

brown; not entirely smooth, but with protuberances sufficiently considerable: and their cellular tissue is in general of a medium consistence between that of the Italian and the Istria galls. These galls yielded a fluid saporific, odorous, a little whitish, and without a drop of oil. After setting, there appeared visible, near its surface, a very small part swimming, which, with agitation, became more conspicuous. Separating this by filtrating paper, and then exposing the paper to a moderate heat as usual, an odorous vapour arose, but less copiously. I have obtained a similar result from another kind of Aleppo galls, which differed from the preceding in being all of a greenish colour.

From these experiments, therefore, it appears that the volatile oil here spoken of is contained in all the above kinds of galls, but not in the same quantity; and it is to be observed, that those called *Istria galls* and *black Istria galls*, which yielded the greatest quantity, are also those which have more odour when pounded.

I have twice repeated the same experiment without the least essential difference*. The odorous fluids, which reddened more or less the paper tinged with turnsole, by containing a greater or less dose of acetic acid, sometimes became limpid in a short time, and sometimes remained turbid many days. Some have had no sensible colour, nor did they blacken the solution of sulphate of iron; others were slightly yellowish, and evinced by means of this sulphate that they contained a small quantity of gallic acid †. Perhaps these last characters were in consequence of some little of the powder of the galls remaining in the neck of the retort, notwithstanding the pains which I took in clearing it.

The bark of oak and the gland seem to contain a very small quantity of our volatile oil, as, having submitted them separately to the same process of distillation ‡, they yielded a fluid some-

* In the second distillation of six ounces of Istria galls I obtained somewhat less than a grain of volatile oil, and a little more than a grain from the same quantity of Istria black galls. It is also to be remarked, that the liquor in the former was less turbid than in the latter, and that the volatile oil separated from the last did not appear in the receiver like drops, but formed on the surface of the liquor a thin film which became more opaque as it cooled. Deyeux from the same quality and quantity of galls did not always obtain the same quantity of extract, but sometimes more and sometimes less

† This liquid, which I filtered and preserved in bottles with ground-glass stoppers, became more or less altered in the course of a month. Such an alteration, and particularly in odour, appeared very conspicuous on comparing it with that recently distilled.

‡ For the distillation of six ounces of bark, eighteen ounces of water were necessary to have some superabundant, and fourteen ounces for the glands.

what

what whitish, and with a slight odour analogous to that of the oil, as has been observed by many of my friends and pupils to whom I have shown it. After settling, there appeared in the bottoms of the bottles in which I had placed these distilled fluids some very small white particles, but the pieces of paper used for filtering them, when exposed to heat, did not yield any sensible odorous vapour.

I believe that I do not deceive myself in saying, that no notice relative to this concrete volatile oil is to be found in Chaptal's Elements of Chemistry, in Deyeux's Memoir* on the nut-galls of the oak, in Fourcroy's System of Chemical Knowledge, in the Chemistry of the *Encyclopédie Méthodique*, nor in Thomson's Chemistry. It is true, Bouillon Lagrange in his *Faits pour servir à l'Histoire de l'Acide gallique*, published in 1806, has observed, that when this acid is extracted from galls by the method of Deyeux, that is by sublimation, it contains a small quantity of volatile oil, which he considered merely as a product of the sublimation; saying, "it cannot be doubted that there is formed in the liquor some acetic acid, which acting on a portion of tannin and extractive matter, constitutes the gallic acid of Scheele; but this combination becomes more intimate, and even varied, by the aid of caloric. We have a proof of it when we obtain this acid by means of sublimation; not only the tannin is decomposed, but the acid remains combined with a volatile oil which is formed †." In confirmation of this, I have not found any mention made of a volatile oil, as a constituent part of the nut-galls of the oak, in any work subsequent to the publication of this memoir; such as, among others, Brugnatelli's Chemistry and Pharmacopœia; Klaproth and Wolff's Chemical Dictionary; the Annotations of Professor Morelli; Thenard's Chemical Treatise, published last year in Paris; and lastly, Sir Humphry Davy, who has recently analysed these galls without speaking of any volatile oil contained in them. The latter found that 500 grains of Aleppo nut-galls yielded 185 grs. of soluble matter, which was composed of tannin 130; gallic acid united with a little extractive matter 31; mucilage and matter rendered insoluble by the evaporation 12; carbonate of lime and a saline substance 12: add to these, the ligneous part when burnt contained much calcareous carbonate ‡.

Notwith-

* *Ann. de Chimie*, xvii. 3. According to this author, Aleppo galls are composed of a mucous body, of a true extractive matter, of a particular kind of resin, of a green colouring part, of gallic acid, and a ligneous tissue. Afterwards he observes that it is possible that other substances may have escaped his research.

† *Ann. de Chimie*, lx. 180.

‡ Thenard, *Traité de Chimie*, iii. 343. *Note by the Translator.* This oil keeps very well in a properly-stopped bottle, is of the consistence and colour

Notwithstanding the silence of so many most respectable writers authorizes me in believing that none before me has extracted from the nut-galls of the oak a volatile concrete oil; yet it is still possible that some chemist, especially a foreigner, may have anticipated me, not only in obtaining it, but in announcing it to the public, without my having any knowledge of the fact. In such an event, I shall always reflect with pleasure on my having rendered more common by this letter the knowledge of a fact certainly not contemptible, inasmuch as it appears to have been unknown to the above chemists as well as to myself.

I am, &c.

Pisa, June 1816. J. Branchi.

LXVII. *On the Atomic Theory. An Extract of* M. H. Gaultier de Claubry, *from the "Journal de Physique" for May* 1817, *page* 392. *Translated, with Remarks, by* W. B.

It often occurs in the sciences, that a discovery rests unknown even in the country which gave it birth, and that a long time afterwards, the same object having drawn attention, men discover a former work written on the same subject. For example, in 1630, John Rey discovered the cause by which lead and tin increase in weight when sufficiently heated, and it was not until the immortal labours of Lavoisier that chance drew attention towards the work of Rey. It might also happen that two scientific men engaged in similar researches should arrive precisely at the same results without knowing any thing of each other's labours*; but, under all circumstances, the discovery of right belongs to the person who had first published on the subject, more especially when the author has not only observed a principal fact, but at the same time developed the consequences which flow from it. Under this point of view, it appears to us incontestable that Mr. Higgins was the first who developed and published the Atomic Theory, and anticipated that of Definite Proportions, which the labours of MM. Proust, Dalton, Berzelius, Gay-Lussac, &c. have afterwards fully established. In the work under our consideration Mr. Higgins claims priority as to the Atomic Theory, the base of which he announced to the public

lour of good old honey, and has evidently the aroma and taste of galls. Placed on paper and exposed to the flame of a wax candle, it instantly melts, and the paper becomes oily and transparent; in this state, when again exposed to the flame, it immediately evaporates, and leaves the paper so clean that one may afterwards write on it with the greatest ease.

* It is possible indeed for two persons to hit upon the same solitary fact or discovery, but not on a new system which engaged an octavo volume. Tr.

in

in 1789, in a work entitled "Comparative View of the Phlogistic and Antiphlogistic Theories," and which Mr. Dalton has on his side presented in his *New System* of Chemical Philosophy, and which generally has been called the *theory of Mr. Dalton*.

The character of so distinguished a philosopher as Mr. Dalton will not allow us to suppose that he acted the part of a plagiarist towards Mr. Higgins. Still however we must in truth say, that the work cited of the latter contains in nearly the same expressions the bases and the principal facts which Dalton brings forward as the foundation of his *theory*. These two *Savans* have, therefore, arrived at the same result; but the work of Higgins remained little known, having appeared at a period when it was almost impossible to understand views so ingenious and novel in the theory of combinations, whilst on the other hand that of Mr. Dalton, dated only a few years back, attracted public attention, and the theory which he expounded had taken his name.

Mr. Higgins (in the Philosophical Magazine) justifies his claim by citing a great number of passages from his old work, in which appear views exceedingly remarkable on the combinations of metals and oxygen; but particularly on the important facts respecting the combinations of azote and oxygen. That work being scarcely known in France, it is not surprising that, on the occasion of the theory proposed by Mr. Dalton, nobody should revert to the labours of Mr. Higgins; but what appears to us very astonishing is, that in England no person ever mentioned any thing about it; but above all, that Dr. Thomson, who knew the work of Mr. Higgins, should say in his Journal, in consequence of a work of Professor Berzelius on determinate proportions, that Mr. Higgins had only made known some remarkable facts on the combinations of gas in definite proportions; but that Mr. Dalton was the first who generalized that doctrine glanced at by Bergman, Cullen, Black, &c., and also determined the weights of the atoms of bodies *.

Mr. Higgins, in drawing the attention of the public to his first work, has decidedly removed all doubts respecting the question of priority; but that does not diminish the importance of the work of Mr. Dalton, who arrived at the same results, and had developed them in so learned a manner.

The theory of definite proportions is one of the most beautiful results to which one could hope chemistry should lead; one cannot arrive at it, like all the discoveries of the human mind, but by a suite of researches and a collection of a great number of

* Dr. Thomson's conduct towards Mr. Higgins on the whole of this business appears very unfair and unjust, as Mr. Higgins himself has shown in preceding numbers of the Philosophical Magazine.—Tr.

correct analyses: but this theory is now supported by so great a number of facts, that there is very little fear of its being erroneous. Perhaps some exceptions will be found to established rules, but they cannot subvert the great mass of facts which serve for its base. The same law of definite proportions extends itself also to animal and vegetable substances, as the interesting labours of M. Chevreul have recently shown; and it is very probable that, in continuing to examine carefully the combinations of all bodies, it will be found that they constantly combine agreeably to the same law.

Mr. Higgins appears to be the first who has considered combinations under this point of view. Unfortunately, as we have already said, his work being little known, his name has never been inscribed amongst the number of those who have engaged themselves on the subject of combinations in definite proportions.

The Atomic Theory is very curious; and although by its nature it may be subject to great variations according to the manner in which we view the composition of bodies, it may be considered as very important.

Here, even Mr. Higgins has proved himself to have conceived and developed the base of that theory at a time when chemistry was scarcely emerged from a chaotic state, and at the moment when the results of Lavoisier had been still contested by so many distinguished philosophers, particularly by Mr. Kirwan in England. This is what renders his ideas the more important, although they may not be so well developed as Mr. Dalton had afterwards done*. We should often estimate the importance of a discovery less by its absolute value than by the state of the science at the moment it was made; and under this point of view, the application which Mr. Higgins made in 1789, of the recent

* I beg leave to differ from the learned author of this article; for, if he more carefully peruses the work of Higgins, he must readily perceive that Dalton brought his doctrine forward in a mutilated state; for he omitted what appears to me to be the most important part of Mr. Higgins's beautiful doctrine; namely, that of the relative forces of attraction of the ultimate particles of bodies to each other, particularly when they are found to be capable of uniting in more proportions than one and one. To make this part of the subject intelligible to common readers, it must be stated, that Mr. Higgins supposed, like many philosophers who had written before him, that the attraction of bodies one to another is mutual; but he also observed that, in chemical science, there were important modifications of this law. Instance: An ultimate particle of hydrogen and an ultimate particle of oxygen united form an atom of water;—here the attraction of both particles is mutual; that is, they attract each other with equal force, and the compound atom is incapable of uniting to any more of its constituent elementary particles, having already arrived at its definite proportions. Again: A particle of sulphur unites to a particle of oxygen with a certain force, the compound is

recent discoveries, to a theory of the intimate composition and combinations of bodies, appears to me to have been extremely happy; at the same time that it may not be correct in some points; that its different parts may not be perfectly connected; in short, that it may be an outline rather than a theory well established:—this is our opinion, but it does not diminish the value of this application.

We regret that the extent of this article does not permit us to cite some passages from the work itself, in which Mr. Higgins has developed his ideas, and which would serve much better than all we could say to make known the importance of this work: but that would lead us far beyond our limits.

Mr. Higgins presents also in the work before us some views on the mode of action of electricity, which he considers as disengaging a portion of the caloric of gases, and by that means producing the phænomena of thunder and lightning, of volcanic eruptions, of earthquakes, of combinations, &c. We do not feel ourselves called upon to follow him on this object, which he offers only as an hypothesis equal to the explanation of the facts which have also been explained in so many different ways.

LXVIII. *On the Ring of Saturn.* *By Count* Laplace.

Two principles are necessary to maintain the ring of Saturn in equilibrio round this planet. One of them relates to the equilibrium of its own parts, which requires that the particles of the surface of the ring should have no tendency to detach themselves; and if we suppose this surface to be fluid, it is maintained in consequence of the different forces by which it is acted upon. Without this, the continual effort of its particles would end by detaching themselves, and the ring would be destroyed, like all the works of Nature which have not in themselves a sufficient cause of stability to resist the action of contrary forces. I have proved in the third book of the *Mécanique Céleste*, that this

is an atom of sulphurous acid: a second particle of oxygen unites so as to produce an atom of sulphuric acid;—the one particle of sulphur and the one of oxygen are combined with greater force than the one of sulphur and the two of oxygen in the atom of sulphuric acid. He extends the same principles to bodies which unite 1 and 3, 1 and 4, and 1 and 5. He instances the different compounds of azote and oxygen in nitrous acids and nitrous gases; and so on, to the union of oxygen to the different metals. These principles being once developed, it is easy to conceive the play of affinities, and the phænomena which are produced during the chemical action of bodies on each other. This part of the doctrine has been demonstrated by means of diagrams and numbers, &c. It is extraordinary that this valuable part of the system should be passed over in silence by those who have engaged themselves on the subject of definite proportions.—Tr.

property

property can only be rendered complete by a rapid motion of rotation of the ring in its own plane, and round its own centre, which is not very distant from that of Saturn. I have also shown, that the section of the ring by a plane perpendicular to itself passing through its centre is an ellipsis, elongated towards this point.

The second principle relates to the suspension of the ring round the body of Saturn. A hollow sphere, and generally a hollow ellipsoid, whose interior and exterior surfaces are similar and concentric, would be in equilibrio round Saturn, whatever might be the point of concavity occupied by the centre of the planet; but this equilibrium would be *indifferent*, that is, being acted upon, it would neither tend to take its primitive state again, nor to remove away;—the slightest cause, such as the action of a satellite, or of a comet, would therefore be sufficient to precipitate the ellipsoid on the planet. The indifferent equilibrium which takes place for a hollow sphere enveloping Saturn, would not exist for a circular zone, surrounding this planet. I have shown in the above cited book of the *Mécanique Céleste*, that if the two centres of the circular ring, and the planet, do not coincide, they will then repel, and the ring will end by being precipitated on Saturn. The same thing would take place whatever might be the constitution of the ring, if it were without any motion of rotation: but if we conceive that it is not similar in all its parts, and so constituted that its centre of gravity does not coincide with that of its figure;—if moreover we suppose it to be endowed with a rapid motion of rotation in its plane, then its centre of gravity would turn round the centre of Saturn, and gravitate towards this point like a satellite, with this difference, that it might move in the interior of the planet: it would then have a permanent state of motion. Thus, the two properties I have mentioned, concur in showing that the ring turns in its plane, on itself, and with rapidity. The duration of this rotation ought to be nearly that of the revolution of a satellite moving round Saturn at the distance of the ring itself; and this duration is about ten hours and a half. Dr. Herschel has confirmed this result by his observations; but how are we to reconcile these facts, and the theory, with the observations of M. Schröter, in which, points of the ring, more luminous than others, have appeared stationary for a long time? I believe we may account for this in the following manner.

Saturn's ring is composed of several concentric rings; with good telescopes two may be perceived very distinctly; but irradiation confounds these into one, in bad telescopes. It is very probable that each of these rings is itself formed of several rings, so that Saturn's ring may be considered as an assemblage of several

several different concentric rings. Such would be the whole of the orbits of the satellites of Jupiter, if each satellite left a permanent light in its path; the partial rings must be like these orbs, differently inclined to the equator of the planet: and then, their inclinations, and the positions of their nodes, changing in periods of greater or less time, embracing several years, their centres must in like manner oscillate round that of Saturn; all this would cause the apparent figure of the whole of these rings to vary. Their motion of rotation would not change this figure sensibly, since it only replaces one luminous part by another situated in the same plane. It is very probable that the phænomena observed by M. Schröter are caused by variations of this kind. But if a point, more or less luminous than the others, be adherent to the surface of one of the partial rings, this point ought to move as rapidly as the ring, and appear to change its position in a few hours. We may suppose with great probability that it is a point of this kind that Dr. Herschel has observed. I request observers furnished with good telescopes to notice the appearances of the ring of Saturn with this view. The variety of these appearances were a great plague to mathematicians and astronomers before Huygens had found out the cause: the ring was first seen by Gallileo in the form of two small bodies adhering to the globe of Saturn; and Descartes, who unfortunately wished to explain all things with his principles of philosophy, attributed, in the third part of his work, the stationary state of these pretended satellites, to Saturn's presenting always the same side to the centre of his vortex. We now know that this state is contrary to the law of universal gravitation; which is a sufficient reason for rejecting the explanation given by Descartes, even although we do not know the precise cause of these appearances. I do not consider the immobility of the ring as less contrary to this great law of Nature; and I do not doubt but that future observations, made with the view I have just pointed out, will confirm the results of the theory, and the observations of Dr. Herschel.

LXIX. *An easy, simple, and infallible Method to force every Fruit-Tree to blossom and to bear Fruit. Translated from the German of the Rev.* George Charles Lewis Hempel *(Secretary to the Pomological Society of Altenburgh in Saxony) by* George Henry Noehden, *LL.D.F.L.S.&c.**

In my early years I saw my father, who was fond of pomology and skilled in that science, cutting a ring on several branches of

* From Transactions of the London Horticultural Society.

trees,

trees, which already were in blossom, for the purpose of producing, by that means, larger fruit than usual. This was not his own invention, but, as far as I recollect, derived from a French journal. Thirty years ago, when I was a boy, I practised this operation, in imitation of him, and thereby obtained larger pears and plums. In repeating this operation of *ringing* the branches, which I did merely for the purpose of getting larger fruit, I observed that the branches so operated upon always bore the next year. By this reiterated appearance I was led to the idea, that perhaps this mode of ringing the bark might be a means of compelling every unproductive branch to yield fruit. With this view, I cut rings upon a considerable number of branches, which as yet showed no blossom; and found by repeating the experiment the truth of my supposition indisputably confirmed by experience.

The application of this experiment, whereby upon every bough or branch fruit may artificially be produced, is very simple and easy, and the mode of proceeding as follows.

With a sharp knife make a cut in the bark of the branch which you mean to force to bear, and not far from the place where it is connected with the stem, or, if it be a small branch, or shoot, near to where it is joined to the larger bough; the cut is to go round the branch, or to encircle it, and to penetrate to the wood. A *quarter of an inch* from this cut, you make a second cut, like the first, round the branch, so that, by both encircling the branch, you have marked a ring upon the branch, a quarter of an inch broad, between the two cuts. The bark between these two cuts you take clean away with a knife, down to the wood, removing even the fine inner bark, which immediately lies upon the wood; so that no connexion whatever remains between the two parts of the bark, but the bare and naked wood appears white and smooth. But this bark-ring, which is to compel the tree to bear, must be made at the right time, that is, when in all nature the buds are strongly swelling or are breaking out into blossom. In the same year a callus is formed at the edges of the ring, on both sides, and the connexion of the bark, that had been interrupted, is restored again without any detriment to the tree, or the branch operated upon, in which the artificial wound soon again grows over.

By this simple though artificial means of forcing every fruit-tree, with certainty, to bear, you obtain the following important advantages:

1. You may compel every young tree of which you do not know the sort, to show its fruit, and decide sooner, whether, being of a good quality, it may remain in its first state, or requires to be grafted.

2. You

2. You may, thereby, with certainty, get fruit of every good sort, of which you wish to see the produce, in the next year.

3. This method may probably serve to increase considerably the quantity of fruit in the country.

The branches so operated upon are hung full of fruit, while the others that are not ringed, often have nothing, or very little on them. This effect is easy to be explained from the theory of the motion of the sap. For, when the sap moves slowly in a tree, it produces fruit-buds, which is the case in old trees; when it moves vigorously, the tree forms wood, or runs into shoots, as happens with young trees.

Though I arrived at this discovery myself, in consequence of trying the same process with a different view, namely to increase only the size of the fruit, but not to force barren branches, that were only furnished with leaf-buds, to bear, this latter application being before quite unknown to me; I will on that account by no means give myself out for the first inventor of this operation; but I was ignorant of the effects to be produced by this method, and only discovered them by repeated experiments of my own, which I made for the promotion of pomology. Frequent experience of the completest success has confirmed the truth of my observations. Nor do I think that this method is generally known; at least, to all those to whom I showed the experiment, the effect produced appeared new and surprising. At all events, that method, supposing it even to be an invention of older date, has, as far as I know, not yet been fully described by any one, and published in print.

LXX. *On the Resistance of Solids; with Tables of the specific Cohesion and the cohesive Force of Bodies.* By Mr. Thomas Tredgold*.

Definitions, &c.

1. Cohesion, or *attraction* of *cohesion* †, is that force by means of which the particles of bodies are held together.

When the particles of a body cohere so slightly that they are easily moved among one another, in every direction, by a very small force, the body is called a *fluid.*

* Communicated by the Author.

† Of the nature of attraction of cohesion, nothing is known; but the phænomena prove the existence of that property of bodies to which the name is applied. It is known that the parts of bodies do cohere, and that, when accidental circumstances are excluded, a determinate force will separate them; and this force being given, the theory of the resistance of solids consists in nothing more than applying the principles of mechanics to determine the power which will destroy that cohesion, when the direction of the power, and the position and magnitude of the body, are given.

When

When the particles of a body can be moved only in a very small degree, without destroying their cohesion, the body is called a *solid*.

2. The *absolute cohesion* of solids is measured by the force necessary to pull them asunder. Thus, if a rod of iron be suspended in a vertical direction, and weights be attached to its lower extremity till the rod breaks, the whole weight attached to the rod, at the time of fracture, would be the measure of its cohesive force, or absolute cohesion.

3. The particles of solid bodies, in their natural state, are arranged in such a manner that they are in equilibrio in respect to the forces which operate on them. Therefore, when any new force is applied, it is evident that the equilibrium will be destroyed, and that the particles will move among themselves till it be restored.

When the new force is applied to pull the body asunder, the body becomes longer in the direction of the force; which is called the *extension;* and its area at right angles to the direction of the force contracts.

When the force is applied to compress the body, it becomes shorter in the direction of the force, which is called the *compression;* and the area of its section, at right angles to the force, expands*.

In either case, a part of the heat, or any other fluid, that occupied the pores or interstices of the body, before the new force was made to act upon it, will be expelled†.

4. The *depth* of a beam, or bar, is the dimension in the direction of the pressure.

Phænomena.

5. All bodies, as far as experience reaches, are extended or compressed by an adequate force.

The extensibility of the most brittle bodies may be rendered sensible by forming them into thin plates. Plates of glass bend considerably when they are only supported at the ends; and this bending could not take place unless the body were both compressible and extensible. Marriotte succeeded in extending some

* The resistance to compression does not arise from any repulsive power in the particles of bodies; indeed, we have not any facts to prove that the particles of any class of bodies actually repel each other;—as, whenever a body is forced into a less space than it occupies in its natural state, one of its constituents, at least, is always expelled; and is restored by a species of capillary attraction, as soon as the external force is removed.

† The disengagement of heat in experiments on the direct cohesion of bodies, appears to have been first noticed by Perronet in his experiments on the strength of iron. Mr. Barlow has lately observed the same phenomena in a greater degree, owing to the large size of the bar that was experimented upon—(Ann. of Phil. x. 311)—as it is obvious that the quantity of heat disengaged must be proportional to the magnitude of the body.

canes

canes of glass, in the direction of their length, which returned to their original length when the weight was removed *.

6. The extension or compression of homogeneous bodies is directly proportional to the force which occasions it; at least in the first degrees of extension or compression.

Thus, if a wire of any metal be extended 1-10th of an inch by a weight of 100 pounds, it would be extended 2-10ths by a weight of 200 pounds; 3-10ths by 300 pounds, &c.†; also, if a body be compressed 1-10th of an inch by 100 pounds, it would be compressed 2-10ths by 200 pounds, &c.

7. In the first degrees of extension or compression, all homogeneous bodies are extended and compressed in an equal degree by equal forces. That is, if a force of 1000 pounds would extend a body 1-10th of its natural length, it would be compressed 1-10th of its natural length by the same force.

It is justly observed by a late writer on this subject, that "so far as this doctrine has been investigated by experiments, its general truth has been amply confirmed; the slight deviations from the exact proportion, which have been discovered in some substances, being far too unimportant to constitute an exception, and merely tending to show that these substances cannot have been perfectly homogeneous, in the sense here attributed to the word ‡." In making experiments on the extension and compression of bodies, the times of action have not often been attended to with a sufficient degree of accuracy. It is well known that a certain time must elapse before the weight produces its full effect upon the body, particularly when the weight is considerable; and from a few experiments of my own, I am inclined to think, that were the weight suffered to produce its full effect at each operation, we should find the extension exactly proportional to the force, even to the time of fracture, and it is most probable that the same observation will apply to compression.

8. When a bar or beam is fixed at one end, in a horizontal position, and a sufficient weight suspended at the other end, the bar will break at the point of support; and the following phænomena will take place.

9. The bar will bend, and the bending will be proportional to the extensibility of the material.

10. The upper side of the bar will be extended, and the lower

* *Traité du Mouvement des Eaux*, Sect. v. Discours ii.

† Some experiments of this kind were made by Emerson,—Mechanics, Prop. 76. Ed. 1773. Bernouilli has attempted to demonstrate that the compression cannot be proportional to the force; but his reasoning applies to extreme cases only; and the result of the solitary experiment he made is completely different from those of every other writer that I have consulted. See his paper in the *Mem. de l'Acad. des Scien.* Paris 1705, p. 179.

‡ Supplement to Encyclop. Britannica, art. *Bridge*, p. 497. 1817.

side

side compressed; but a horizontal stratum of particles, at or near the middle of the depth, will neither be extended nor compressed. Hence the line representing this stratum, in the section of the bar, is called the *neutral line*.

11. The extension and compression of any part of the section is proportional to its distance from the neutral line.

The quantity of extension and compression, and the position of the neutral line, may be observed in some soft woods, with a considerable degree of accuracy, by drawing two vertical lines, very near to each other, against one of the vertical sides of the bar at the place of fracture, before the weight is applied.

Sometimes the parts that have been extended may be distinguished from those which have been compressed by the fracture, and in fibrous substances, by stopping the descent of the bar before the fracture is completed, the position of the neutral line may be observed. The last is the best method.

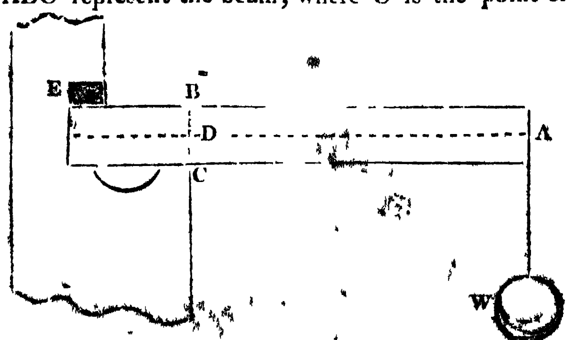
Du Hamel made some experiments on bars of willow, with the view of determining the position of the neutral line, by cutting the bars to different depths, with a saw, on the compressed side*, but this method is not susceptible of much accuracy.

12. From many observations, I have found that when the section is rectangular or circular, the neutral line passes through the centre of gravity of the section, or extremely near it. Or, more generally, that when the neutral line divides the section into two parts, that are equal and similar, it passes through its centre of gravity;—and that in triangular sections the distance of the centre of gravity from the vertex is about seven-tenths of the height.

On the transverse Strength of Beams when the neutral Line divides the Section into two Parts that are equal and similar.

13. *Prop.* To determine the strength of a beam fixed at one end to support a weight suspended at the other end.

Let ABC represent the beam; where C is the point of sup-



port, BC the place of fracture, and D the place of the neutral line.

* Transport du Bois, p. 419.

Then,

Then, the weight W will act with the leverage AD; and D will be the centre of motion. Now it is evident, that when the weight at A causes the beam to move round the centre D, the upper side at B will be extended and the lower side at C compressed (art. 10); and the strain on any part above or below the neutral line will be directly as its distance from that line; and the extension or compression will be as the strain (arts. 6 and 11).

Also, as the compressed part is the fulcrum which supports the lever till the extended part is torn asunder; and bodies are extended and compressed in equal degrees by equal forces (art. 7); therefore, the neutral line must divide the section into two equal and similar parts, because the forces on each side of the neutral line must be equal*.

Put l = the length of the beam AD;
a = the distance of the neutral line from the upper side;
y = the breadth of the beam;
f = the cohesive force of an unit of the area;
$y\dot{x}$ = a variable part of the section of fracture;
and x = its distance from the neutral line.

Then the force of any variable part of the section is as its distance from the neutral line, or $a : x :: f : \frac{fx}{a}$; and as its area $y\dot{x}$; hence, its whole force is $= \frac{fyx\dot{x}}{a}$ which being multiplied by its distance from the centre of motion, gives $\frac{fyx^2\dot{x}}{a}$ = the fluxion of the force exerted by the part of the beam above the neutral line. But the forces on each side of the neutral line are equal; therefore, in the case of equilibrium, we have

$$\text{Fluent of } \frac{2fyx^2\dot{x}}{a} = l\text{W}; \text{ or flu. } \frac{2fyx^2\dot{x}}{la} = \text{W}.$$

* Most of the writers on the strength of materials have considered the point of support C, as the fulcrum to the extending forces, but the support is a fulcrum only in respect to a force at E;—this mistake arises from the absurd method of demonstrating the properties of the lever, by supposing it to be an inflexible line; had the properties of the lever been sought for in the lever itself, this could never have happened.

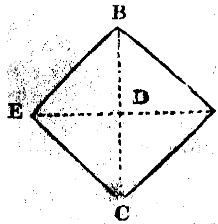
In the natural order of science, the resistance of beams should occupy the place that is now assigned to the doctrine of the lever, as its properties are merely so many corollaries which naturally flow from propositions in the doctrine of the resistance of beams. For the strains excited in a beam may be investigated directly by means of the properties of the parallelogram of forces, without referring at all to those of the lever; and the properties of a beam considered as a lever, may be deduced from the strains excited in the beam, which appears to be the only legitimate mode of demonstrating its properties. See art. *Carpentry*, p. 167. Supplem. Encyclo. Brit. 3d ed. 1811; or art. *Carpentry*, p. 633, New Supp. 1817.

 14. When

14. When the beam is rectangular, and its depth $= d$; $a = \frac{d}{2}$; and making $y = b$, we have $\frac{2fyx^2\dot{x}}{la} = \frac{4fbx^2\dot{x}}{ld}$; the fluent of which is $\frac{4fbx^3}{3ld}$, and when $x = \frac{d}{2}$, it becomes $\frac{fbd^2}{6l} =$ $W =$ the transverse strength of the beam.

15. When a beam is supported at both ends, its strength to bear a weight in the middle of its length, is to that of a beam of the same length, supported at one end, as 4 is to 1. Hence $\frac{2fbd^2}{3l} = W =$ the strength of a beam supported at both ends.

16. When the beam is square, and the force acts in the direction of its diagonal BC; EF will be the neutral line, $BD = a$, and $EF = 2a$. Also, by sim. trians. $a : a - x :: 2a : 2(a - x) = y$.



Therefore $\frac{2fyx^2\dot{x}}{la} = \frac{4f(ax^2\dot{x} - x^3\dot{x})}{la}$;

and, when $x = a$, the fluent is $\frac{fa^3}{3l}$, or, because $2a = d =$ the diagonal; $\frac{fd^3}{24l} =$ $W =$ the strength of the beam supported at one end.

And when a beam of the same length is supported at both ends, $\frac{fd^3}{6l} = W =$ the strength of the beam.

17. When the beam is a cylinder, and its radius $= r$; $a = r$, and, by the property of the circle, $y = 2\sqrt{(r^2 - x^2)}$.

Therefore $\frac{2fyx^2\dot{x}}{la} = \frac{4fx^2\dot{x}\sqrt{(r^2 - x^2)}}{lr}$; and its fluent when $x = r$, and $p = 3{\cdot}141 = 5$, &c. is $\frac{fpr^3}{4l} = W =$ the strength of a cylinder supported at one end; or, $\frac{fpr^3}{l} = W =$ the strength supported at both ends.

18. From the preceding examples it appears, that when the length and the areas of the sections are the same, the transverse strengths of the beams will be nearly in the following proportion; viz.

The strength of a square beam being unity, or ..	1·000
The strength of the same beam when the force is in the direction of the diagonal, will be	0·7071
And that of a cylinder	0·846

It is not, however, to be expected that these proportions will perfectly agree with experiments, unless the material be homogeneous. For this reason, timber is very unfit for the purpose of making experiments to establish a pure mechanical theory.

When

When a beam is compressed in the direction of its length, the deflexion is always in the direction of the diagonal, when the section is nearly square; this has been shown by the experiments of Girard, Lamandé, and Navier; and it is only a modification of the transverse strain, which confirms the general principles of the theory, and at the same time shows that a cylinder is the best form for an insulated post *.

19. When the neutral line divides the section into two parts, that are neither equal nor similar; assume the distances of the upper and lower sides of the beam from the neutral line, and find the force exerted on each side of the neutral line by art. 13. Make those forces equal to each other, and from this equation the distance of the neutral line from the upper side may be obtained; which being substituted for the assumed distance in the expression for the force of the part above the neutral line, will give half the strength of the beam.

In this manner the rule for the strength of a triangular beam has been found, which is inserted in the Philosophical Magazine, vol. xlvii. p. 22.†

20. As the cohesion of their parts not only serves to characterize different substances, but also to determine their relative value in the various uses to which they may be appropriated; I have endeavoured to collect from various sources, the best experiments on this important subject; and to present them in the form which appeared to me best adapted to render them capable of universal application.

For this purpose they are reduced to a common standard, from which, by a simple operation, they may be reduced to any kind of weights or measures.

I have adopted plate glass as a standard, because it is a substance tolerably uniform in its nature, its defects may be readily perceived, and it is not used where its strength need be calculated; and therefore, the strengths of different kinds of it will not be required. The experiments were made on the transverse strength, and the direct cohesion was calculated by art. 14. The pieces were loaded by letting sand run slowly into the scale till they broke. The results were regular, and the highest was taken.

* This remark extends only to the form of the section.

† The rule for the resistance to crushing (Phil. Mag. xlvii. 22) was an attempt to apply the principles which M. de Prony has used in his investigation of the push of the earth against retaining walls to the resistance to crushing—neglecting the effect of the cohesion in determining the angle of fracture. Since that time I have found that the angle of fracture will vary with the cohesion; therefore any rule where it is neglected can only be a rude approximation.—It may not be improper here to state, that in M. de Prony's investigation of the push of the earth, one very material element is omitted; viz. the friction against the back of the wall. The existence and effect of this friction may readily be seen from the results of Col. Pasley's experiments. "Course of Military Instruction," iii. 563.

 Experi-

Experiments on the direct cohesion of bodies require to be made with much care, in order to obtain correct results, particularly when they are brittle; and it is also very probable that the strength of bodies, to resist being torn asunder, is not exactly as their areas. For if the cohesion be as the area, it does not follow that the strain excited by a force tending to pull the body asunder should be in the same proportion. Indeed, Count Rumford's experiments led him to conclude that the strength is not in the simple proportion of the area*, and Perronet found that when a bar of iron was strained in the direction of its length, a very slight incision made with a file on one side, sometimes reduced its strength more than one-half; which could not have been the case had the strain been equally diffused over the section †.

Hence I conclude, that the length being taken so that the deflexion ‡ does not sensibly affect the result, the cohesion calculated from the transverse strength, is as near or nearer the real cohesion than that obtained by pulling the body asunder.

Morveau very justly observes that the *maximum* cohesion of the metals ought not to enter into the calculations of the artist who employs them, as it is known that accidental imperfections always accelerate the rupture in a greater or less degree, and render it necessary to augment their dimensions; but it is not less true that their proportional tenacity being known, it would assist the artist in his choice, and furnish the valuable means of ascertaining the degree of purity, and the qualities which are imparted to them in the course of their manufacture §.

It will be seen from the following Tables, that the cohesive force of metals is much increased by wire-drawing, rolling, and hammering; and that the strength of woods of the same kind is extremely variable, depending on the nature of the soil, the situation, and the climate where they are grown.

A complete table ought to exhibit the limits of these variations, and might be made to contain much useful information for the planter and landed proprietor,—but not till experimentalists give better descriptions of the specimens they try, than they have hitherto done.

In the following tables the specific cohesion of plate glass is denoted by unity; and,

The cohesive force of a square inch is 9420 pounds avoirdup.
.. .. .. of a squ. millimetre is 6·62 kilogram. nearly.
.. .. .. of 25 squ. centim. is 16543·625 kilograms.
.. .. .. of a square pouce is 9906·26 Paris pounds.

Hence the specific cohesion of a substance multiplied by one of these numbers will give the cohesion in the corresponding weights and measures.

* Phil. Mag. x. 51. † Œuvres de Gauthey, ii. 150.
‡ On the effect of the deflexion, see Phil. Mag. xlvi. 191.
§ *Ann. de Chimie*, lxxi. 190.

TABLES OF THE COHESIVE FORCE OF SOLID BODIES.

TABLE I.—METALS.

(*h*) and (*l*) mark the highest and lowest result which Muschenbroëk obtained from each kind of iron.

Metals.	Specific Cohesion. Glass 1.	Cohesive Force of a square Inch in lbs. avoir.	Specific Gravity.	Hardness.	Authority.
STEEL.			†		
Razor temper......	15·927	150,000	7·78 to	§	Muschenbroek, Encyclo. Brit. art. *Strength.*
Soft	12·739	120,000	7·84	11	Idem.
IRON.					
Wire...............	12·004	113,077	from 7·6 to 7·8 †	from 8 to 9 †	Sickingen, Ann. de Chimie xxv. 9.
German bar, mark BR (*h*)........	9·880	93,069			Muschenb. Int. ad Phil. Nat. i. 426.
Swedish bar (*h*)	9·445	88,972			Idem.
German bar, mark L (*h*)	9·119	85,900			Idem.
Wire	9·108	85,797			Buffon, Œuvres de Gauthey ,i
Bar	8·964	84,443			Emerson, Mechanics, 115.
Liege bar (*h*)	8·794	82,839			Muschenb. Int. ad Phil. Nat. i. 426.
Spanish bar	8·685	81,901			Idem.
Bar	8·581	80,833			Soufflot, Rondelet's L'Art de Bâtir, iv. 500.
Bar	8·492	80,000			Edin. Encyclo. art. *Bridge*, 544.
Oosement bar (*h*) ..	8·142	76,697			Muschenb. Intr. ad Phil Nat. i. 426.
Cable	7·752	73,024			Annals of Phil. vii. 320.
German bar, mark L (*l*)	7·382	69,538			Muschenb. Intr. ad Phil Nat. i. 426.
German bar, common	7·339	69,133			Idem.
Swedish bar, Oosement bar (*l*)	7·296	68,728			Idem.
Bar of best quality ..	7·006	66,000			Rumford. Phil. Mag. x. 51.
Liege bar (*l*)	6·621	62,369			Muschenb. Intr. ad Phil. Nat. i. 426.
German bar, mark BR (*l*)........	6·514	61,361			Idem.
Bar*	6·480	61,041			Perronet, Œuv. de Gauthey, ii. 154.
Bar of good quality	5·839	55,000			Rumford, Phil. Mag. x. 51.
Cable	5·787	54,513			Annals of Phil. x. 311.
Bar, fine-grained....	5·306	49,982			Rondelet, L'Art de Bâtir, iv. 502.
—— medium fineness	3·618	34,081			Idem.
—— coarse-grained	2·172	20,460			Idem.
CAST IRON.					
French .... ‡	7·470	70,367			Navier, Œuv. de Gauthey, ii. 150.
German..........	7·250	68,295	7·807		Muschenb. Intr. ad Phil. Nat. i. 417.
French, soft ...... ‡	6·754	63,622			Rondelet, L'Art de Bâtir, iv. 514.
English .... ‡	5·520	52,000			Banks, Gregory's Mechan., i. 129. Ex. i.
French .... ‡	5·412	50,981			L'Ecole des Ponts, &c. Gaut. ii. 150.
—— .... ‡	4·540	42,666			Gauthey, Œuvres, ii. 150.
English, soft ...... ‡	4·334	40,824		§	Banks, Greg. Mech. i. 128. Ex. iii.

† Kirwan, Elem. Miner. ii. 155.
* This is the mean result of thirty-three experiments.
‡ Calculated from experiments on the transverse strength, by arts. 14 and 15.
§ Yielded to the file without difficulty.

TABLE I.—(continued.)

Metals.	Specific Cohesion. Glass 1.	Coh. Force of a squ. Inch in lbs. avoir.	Specific Gravity.	Hardness.	Authority.
CAST IRON.					
French gray .... ‡	4·000	37,680			Rondelet, L'Art de Bâtir, iv. 514.
Gray, of Cruzot, 2d fusion. . . ‡	3·257	30,680			Ramus, Gauthey, ii. 150,
Gray, of Cruzot, 1st fusion ‡ . .	3·202	30,162			Ramus, Gauthey, ib.§
COPPER.					
Wire.............	6·606	61,228			Sickingen, Ann. de Chimie, xxv. 9.
Cast, Barbary	2·396	22,570	8·182	8†	Muschenb. Intr. ad Phil Nat. i.417.
——, Japan.......	2·152	20,272	8·726		Idem.
PLATINUM.					
Wire	5·995	56,473	20·847	8‖	Morveau, Ann de Chimie, xxv. 8.
Wire	5·625	52,987			Sickingen, Ann. de Chim. xxv. 9.
SILVER.					
Wire.............	4·090	38,257		7‖	Sickingen, Ann. de Chim. xxv. 9.
Cast	4·342	40,902	11·091		Muschenb. Intr. ad Phil. Nat. i. 417.
GOLD.					
Wire.............	3·279	30,888		6†	Sickingen, Ann. de Chim. xxv. 9.
Cast	2·171	20,450	19·238		Muschenb Intr. ad Phil Nat. i. 417.
TIN.					
Wire.............	0·7568	7,129			Morveau, Ann. de Chim lxxi. 194.
Cast, English block	0·706	6,650			Muschenb. Intr. ad Phil Nat. i.417.
——, idem	0·565	5,322	7·295	6†	Idem.
——, Banca	0·3906	3,679	7·2165		Idem.
——, Malacca ...	0·342	3,211	6·1256		Idem.
BISMUTH.					
Cast	0·345	3,250	9·810	7‖	Muschenb Intr. ad Phil Nat. i.417.
——	0·3193	3,008	9·926		Idem. i. 454.
ZINC.					
Wire.............	2·394	22,551			Morveau, Ann de Chim lxxi. 194.
Patent sheet	1·762	16,616		6†	By my trial.
Cast, Goslar, from	0·3118	2,937	7·215		Muschenb. Intr. ad Phil. Nat. i.417.
to ..	0·2855	2,689			
LEAD.					
Milled	0·3533	3,328	11·407		By my trial.
Wire.............	0·334	3,146	11·348		Muschenb. Intr. ad Phil Nat. i.452.
Wire.............	0·274	2,581	11·282	5‖	Idem.
Wire.............	0·2704	2,547			Morveau, Ann. de Chim lxxi. 194.
Cast, English	0·094	885	11·479		Muschenb. Intr ad Phil. Nat. i.452.
Antimony, cast ..	0·1126	1,060	4·500	6†	Muschenb. Intr ad Phil Nat. i.417.

§ In the operation of casting, the surface of the iron always becomes much harder, and is more tenacious than the internal parts; hence, the strength of a small specimen is always greater than that of a large one. Those of M. Ramus, however, are unexceptionable in this respect, as the area of the section at the place of fracture was above nine square inches.

N.B. When the specific gravity is not referred to a separate authority, it is to [illegible]dered that of the specimen of which the cohesive force is given.

[illegible]n's Miner. vol. ii. ‖ Thomson's Chemistry, vol. i.

TABLE II.—ALLOYS.

Alloy of	Parts.		Parts.	Specific Cohesion. Glass 1.	Cohesion of square inch in lbs. avoir.	Specific Gravity.	Authority.
Gold	2	Silver	1	2·972	28,000		Musch. Encyclop. Brit. art.
Gold	5	Copper	1	5·307	50,000		Idem. [*Strength.*
Silver	5	Copper	1	5·148	48,500		Idem.
Silver	4	Tin	1	4·352	41,000		Idem.
Brass		....		4·870	45,882		Muschenb., Colson, i. 242.
Copper	10	Tin	1	3·407	32,093		Musch. Intr. ad Phil. Nat.
Copper	8	Tin	1	3·831	36,088		Idem. [i. 428.
Copper	6	Tin	1	4·687	44,071		Idem.
Copper	4	Tin	1	3·794	35,739		Idem.
Copper	2	Tin	1	0·108	1,017		Idem.
Copper	1	Tin	1	0·077	725		Idem.
Tin, English,	10	Lead	1	0·733	6,904		Musch. Intr. ad Phil. Nat.
Tin, ———,	8	Lead	1	0·841	7,922		Idem. [i. 438.
Tin, ———,	6	Lead	1	0·849	7,997		Idem.
Tin, ———,	4	Lead	1	1·126	10,607		Idem.
Tin, ———,	2	Lead	1	0·793	7,470		Idem.
Tin, ———,	1	Lead	1	0·751	7,074		Idem.
Tin, Banca,	10	Antimony ..	1	1·187	11,181	7·359	Musch. Intr. ad Phil. Nat.
Tin, ———	8	Antimony ..	1	1·049	9,881	7·276	Idem. [i. 442.
Tin, ———,	6	Antimony ..	1	1·341	10,632	7 228	Idem.
Tin, ———,	4	Antimony ..	1	1·431	13,480	7·192	Idem.
Tin,	2	Antimony ..	1	1·277	12,029	7·105	Idem.
Tin, ———,	1	Antimony ..	1	0·338	3,184	7 060	Idem.
Tin, ———,	10	Bismuth	1	1·347	12,688	7 576	Musch. Intr. ad Phil. Nat.
Tin, ———,	4	Bismuth	1	1·772	16,692	7 613	Idem. [i. 443.
Tin, ———,	2	Bismuth	1	1·488	14,017	8·076	Idem.
Tin, ———,	1	Bismuth	1	1·276	12 020	9·146	Idem.
Tin, ———,	1	Bismuth	2	1·063	10,013	8·58	Idem.
Tin, ———,	1	Bismuth	4	0.836	7,875	9 009	Idem.
Tin, ———,	1	Bismuth	10	0·411	3,871	9·439	Idem.
Tin, ———,	10	Zinc, Indian,	1	1·371	12,914	7·288	Musch. Intr. ad Phil. Nat.
Tin, ———.	2	Zinc	1	1·595	15,025	7·000	Idem. [i. 444.
Tin, ———,	1	Zinc	1	1·682	15,844	7 321	Idem.
Tin, ———,	1	Zinc	2	1·701	16,023	7 100	Idem.
Tin, ———,	1	Zinc	10	0·602	5,671	7·150	Idem.
Tin, English,	1	Zinc, Goslar	1	0·958	9,024		Musch. Intr. ad Phil. Nat.
Tin, ———,	2	Zinc.	1	1·164	10,964		Idem. [i. 446.
Tin, ———,	4	Zinc	1	1·089	10,258		Idem.
Tin, ———,	8	Zinc.	1	1·126	10,607		Idem.
Tin, ———,	1	Antimony ..	1	0 154	1,450	7·000	Musch. Intr. ad Phil. Nat.
Tin, ———,	3	Antimony ..	2	0·338	3,184		Idem. [i. 448.
Tin, ———,	4	Antimony ..	1	1·202	11,323		Idem.
Lead, Scotch,	1	Bismuth . ..	1	0·777	7,319	10·931	Musch. Intr. ad Phil. Nat.
Lead, ———,	2	Bismuth . ..	1	0·620	5,840	11·090	Idem. [i. 454.
Lead, ———,	10	Bismuth . ..	1	0·300	2,826	10 827	Idem.

TABLE III.—WOODS.

Woods.	Specific Cohesion. Glass 1.	Cohesion of a square inch, in lbs. avoir.	Specific Gravity.	Authority.
Lance-wood .... ‡	2·621	24,696	1·022	Layman, Nich. Journal, xxxv. 54.
Locust-tree	2·185	20,582		Muschenb. Intr. ad Phil. Nat. i. 415.
Jujube (Ziziphus)	2·008	18,915		Idem, ... i. 414.
Ash (Fraxinus).				
Red, seasoned .. ‡	1·899	17,892	0·812	Layman, Nich Journal, xxxv. 54.
Ash	1·804	17,000		Barlow, Rees's Cyclop. art. *Strength.*
White, seasoned ‡	1·509	14,220	0·685	Layman, Nich. Journal, xxxv. 54.
Ash	1·274	12,000		Muschenb. Ency. Brit, art. *Strength.*
Oak (Quercus).				
Oak .... ‡	1·891	17,820		Emerson's Mechanics, 114. Ed. 1773.
—, highest result ‡	1·861	17,532		Banks, Gregory's Mechan. i. 127.
—, ...	1·836	17,300		Muschenb. Ency. Brit., art. *Strength.*
Dry, cut 4 years ‡	1·707	16,079		Duhamel, Transport du Bois, p. 212.
Provence, season.* ‡	1·559	14,685	1·164	Idem, ... 434.
English, seasoned, ‡	1·509	14,220	0·880	Layman, Nich. Jour. xxxv. 54.
Oak	1·481	13,951		Rondelet, L'Art de Bâtir, iv. 65.
French, season.† ‡	1·450	13,659		Duhamel, Trans. du Bois, 213.
Provence, season.‖ ‡	1·444	13,602	0·828	Idem, ... 423.
Provence, season. young .... ‡	1·363	12,839	0·771	Idem, ... 230.
Oak, dry ...... ‡	1·274	12,000		Muller, Pract. Fortification, 76.
Baltic, seasoned ‡	1·211	11,412	0·673	Layman, Nich. Jour. xxxv. 54.
Oak, lowest result ‡	1·146	10,800		Banks, Gregory's Mech. i. 127.
——, ... ‡	1·107	10,428		Belidor, Scien. des Ingen. 319. Ed. 1813.
English, ... ‡	1·085	10,224		Beaufoy, Ann. Phil. ix. 286. Ex. No. 4.
Oak, ... ‡	1·076	10,136		Belidor, Scien. des Ing. 321. Ed. 1813.
French, unseason. ‡	1·060	9,985	1·068	Buffon, Mém. de l'Acad. Paris, 1741, [329.
White American, seasoned .. ‡	1·009	9,504		Layman, Nich. Jour. xxxv. 54.
Oak, ... ‡	1·009	9,504		Barlow, Rees's Cyclo. art. *Strength.*
French, unseason. ‡	0·960	9,043		Buffon§, Mém. de l'Acad. Paris, 1741, [328.
Oak	0·955	9,000	0·774	Barlow, Rees's Cyclop. art. *Strength.*
English .... ‡	0·936	8,820		Beaufoy, Ann. of Phil. ix. 297. Ex. 10
Dantzic .... ‡	0·818	7,704		Idem, ... 283. Ex. 19.
Beech (Fagus sylvaticus) ..	1·880	17,709		Muschenb. Intro. ad Phil. Nat. i. 415.
Arbutus, from..	1·845	17,379		Idem, ... 414.
——, to..	0·814	7,667		
Orange (Aurantium)	1·764	16,616		Idem.
to..	1·629	15,345		
Bay (Laurus) ...	1·547	14,572		Idem.
to..	1·085	10,220		

* Its colour brown; and it was hard and large-veined.

† This specimen lay six months in water after it was cut, and was afterwards dried. When the trial was made it had been cut four years.

‖ Middle-aged timber, fine-veined, light and pliant.

§ To find the distance between the supports, in Buffon's experiments, I deducted one-twelfth of the length of the piece, according to the notice he has given in the beginning of his memoir; the two results given are the highest and lowest from the eight-feet lengths.

TABLE

TABLE III.—(continued.)

Woods.	Specific Cohesion Class I.	Cohesion of a square Inch in lbs. avoir.	Specific Gravity.	Authority.
TEAK (Tectona grandis).				
Java, seasoned. . ‡	1·509	14,220	0·697	Layman, Nich. Journ. xxxv. 54.
Pegu, seasoned,	1·400	13,194	0·619	Idem.
Malabar, seasoned ‡	1·[illegible]	13,140	0·658	Idem.
Alder (Bet. Alnus)	1·506	11,186		Muschenb. Intr. ad Phil. Nat. i. 414.
Mulberry (Morus)	1·492	14,054		Idem.
	1·221	11,501		
Elm (Ulmus) .	1·432	13,489		Idem.
FIRS (Pinus).				
Pitch pine, . . . ‡	[illegible]398	13,176		Layman, Nich. Journ. xxxv. 54.
Fir	1·380	13,000		Barlow, Rees's Cyclop. art. *Strength.*
Fir (strongest) . ‡	1·318	12,420		Banks, Gregory's Mech. i. 127.
Pitch pine, . . ‡	1·284	12,096		Beaufoy, Ann. of Phil. ix. 304, 305.
Pine (Pin du Nord), . . ‡	1·264	11,913		Duhamel, Transport du Bois, 460.
Larch (Pinus Larix), . . . ‡	1·177	11,093	0·636	By my trials †
Fir, strong red, . ‡	1·172	11,040		Muller, Pract. Fortification, 76.
Fir, Memel, season ‡	1·154	10,876		By my trial
Fir, Russian . . . . ‡	1·06[illegible]	10,005	0·459	Layman, Nich. Journ. xxxv. 54.
Fir	1·061	10,000		Barlow, Rees's Cyclop. art. *Strength.*
Fir . . . . ‡	1·0[illegible]9	9,792		Idem.
Fir, Riga . . . . . . ‡	0·963	9,072		Beaufoy, Ann. of Phil. ix. 290, 291.
Fir, American . ‡	0·942	8,874	0·416	Layman, Nich. Journ. xxxv. 54.
Fir	0·903	8,[illegible]		Muschenb. Intr. ad Phil. Nat. i. 414.
—, yellow deal ‡	0·900	8,478	0·472	Layman, Nich. Journ. xxxv. 54.
Fir, weakest . ‡	0·879	8,280		Banks, Gregory's Mech. i. 127.
Larch, Scotch, seasoned . ‡	0·837	7,888	0·496	Layman, Nich. Journ. xxxv. 54.
Pitch pine	0·830	7,818		Muschenb. Intro. ad Phil. Nat. i. 414.
Larch, Scotch, very dry, . . ‡	0·745	7,020	0·470	By my trial, very young wood, the lateral cohesion between the annual rings was 1760 lbs. per inch superfi.
Fir, Scotch (P. sylvestris) ‡	0·711	6,600	0·550	By my trials, it was seasoned.
Fir, white deal . . ‡	0·455	4,290	0·498	By my trials, very dry.
Sissor, of Bengal ‡	1·3[illegible]5	13,140	0·[illegible]89	Layman, Nich. Journ. xxxv. 54.
Saul, of Bengal ‡	1·37[illegible]	[illegible]2,9[illegible]0	0·962	Idem.
Plum, (Prunus)	1·357	12,782		Muschenb. Intr. ad Phil. Nat. i. 415.
—— . . . . . to	1·205	11,3[illegible]1		
Willow (Salix) .	1·357	12,78[illegible]		Idem.
Willow, dry . . . . ‡	0·8098	7,626		Duhamel, Trans. du Bois, 419.

† The specimen was taken from the middle between the centre and sapwood, about three feet from the root;—the diameter of the tree was nearly 18 inches, and the wood from which the specimen was taken had been cut into two-inch planks about six months. The wood was tough, hard, and weighed 39¾ pounds per cube foot;—the distance of the annual rings was about an eighth of an inch, and the wood appeared silky when planed. It was grown upon the Duke of Athol's estate at Blair, in Scotland; and the experiments were made at the desire of William Atkinson, Esq. Architect to the Ordnance, &c.

TABLE

TABLE III.—(continued.)

Woods.	Specific Cohesion. Glass I.	Cohesion of a square inch in lbs. avoir.	Specific Gravity.	Authority.
MAHOGANY (Swietenia).				
Spanish, ...‡	1·283	12,186	0·753	Layman, Nich. Journ. xxxv. 54.
Citron (Citreum) ..	1·357	12,782		Muschenb. Intro. ad Phil. Nat. i. 414.
to..	0·868	8,176		Idem.
CHESNUT, Sweet, (Fagus castanea.)				
100 years in use ‡	1·291	12,168	0·877	Layman, Nich. Journ. xxxv. 54.
Jasmine (Jasminum)	1·276	12,020		Muschenb. Intro. ad Phil. Nat. i. 414.
to..	1·248	11,756		
Pomegranate (Punica) ..	1·221	11,501		Idem.
to..	0·882	8,308		
Tamarisk (Tamariscus) ..	1·194	11,247		Idem.
to..	0·732	6,895		
MAPLE (Acer).				
Norway .... ‡	1·123	10,584	0·793	Layman, Nich. Journ. xxxv. 54.
Elder (Sambucus)	1·086	10,230		Muschenb. Intro. ad Phil. Nat. i. 415.
Lemon (Limon) .	1·004	9,457		Idem. .. .. .. 414.
Quince (Cydonia)	0·841	8,822		Idem.
to..	0·624	5,878		
Cypress (Cupressus)	0·732	6,895		Idem.
to..	0·542	5,105		
Poplar (Pop. alba)	0·705	6,641		Idem.
to..	0·488	4,596		
Poplar (P. nigra) lateral cohesion of the annual rings ..‡	0·189	1,782	0·421	By my trials—the specimens very dry.
Cedar	0·528	4,973		Muschenb. Intro. ad Phil. Nat. i. 414.

‡ Those to which this mark is added were calculated from experiments on the transverse strength.

TABLE IV.—MISCELLANEOUS SUBSTANCES.

Substances.	Specific Cohesion. Glass 1.	Cohesion of a square Inch in lbs. avoir.	Specific Gravity.	Authority.
Hemp fibres glued together	9·766	92,000		Rumford, Phil. Mag. x.
Paper strips glued together	3·184	30,000		Idem.
Ivory	1·765	16,626		Muschenb. Intro. ad Phil. Nat. i. 463.
Slate, Welsh, (clay slate) .‡	1·358	12,800		By my trials.
Plate-glass ....‡	1·000	9,420	2·453	By my trials.
Marble (white) ‡	0·955	9,000		Robison, Gregory's Mech. i. 129.
Horn of an ox ..	0·950	8,949		Muschenb. Intro. ad Phil. Nat. i. 463.
Whalebone	0·814	7,667		Idem.
Bone of an ox	0·559	5,265		Idem.
Hard stone† of Givry .....‡	0·230	2,166	2·357	Gauthey, Roz. Journ. de Physiq. iv. 413.
Portland stone, (compact lime-stone). .....‡	0·083	784		By my trials.
Soft stone* of Givry, ...‡	0·041	385	2·071	Gauthey, Roz. Journ. de Physiq. iv. 313.
Brick from	0·031	300		Coulomb, Young's Nat. Phil. ii. 174.
—— to	0·030	280		By my trial,—colour deep-brick red, brittle.
Brick from Dorking, ...‡	0·029	275		
Stone, homogeneous white, of a fine grain	0·022	207		Coulomb, Œuvres de Gauthey, i. 277.
Plaster of Paris...	0·0077	72		Rondelet, L'Art de Bâtir, i. 314.
Mortar of sand and lime, 16 years made ..	0·0054	50		Idem.

In our inquiries respecting the laws which regulate the phænomena of nature, we must always exclude certain circumstances which are not necessary, nor do not always accompany the phænomena.

This principle of exclusion is one of the most prominent fea-

† This stone was hard, of a red colour, and the beds distinctly marked.

* This stone was white, rather soft, and the beds not distinctly marked. These numbers were calculated from experiments on the transverse strength, because the experiments which Gauthey made on the suspending strength are so extremely irregular. He appears to have been aware of the principal cause, which was owing to his mode of fixing the pieces. The results of Gauthey's experiments on the *suspending* strength of stones have been, by mistake, copied as experiments on *crushing*, by Professor Robison, in his article *Strength of Materials*, Encyclo. Brit. See 4th edition, p. 759. This mistake has been copied by some other writers from the article above mentioned, and among others by the writer of the article *Strength*, Rees's Cyclopædia.

Gauthey's experiments on crushing are much more numerous and regular. See Rozier's Journal de Physique, iv. 406.

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tures of Bacon's method of reasoning *; and has been the guide of Newton in his noble discoveries.

In the resistance of solids, we must consider them homogeneous—that they may be extended and compressed in equal degrees by equal forces and proportionally by proportional forces, at least till it is shown by unexceptionable experiments on homogeneous bodies, that these principles are not consistent with the phænomena.

The effect of the deflexion should not be excluded in a perfect theory; but it has been omitted in calculating the preceding tables to save calculation, as it doubles the labour, while the corrections from introducing are too trifling to sensibly affect the result. Besides, such niceties are not needed for practical purposes, where simplicity is much more esteemed.

Timber is nót homogeneous; and therefore it is not a proper material to verify the theory. I have found the transverse strengths of two pieces, of the same size, cut from the side of each other, to be as 9 to 12. Duhamel found pieces of the same size, and from the same zone, to be as 57 to 66 †;—these different results were caused by varying the position of the annual rings. The difference of specimens, from the same tree, both in weight and cohesive force, has been shown by Buffon ‡. Now when such differences are found in the same tree, is it wonderful that experiments, made in different countries, on wood of different ages, seasoned by different methods, and grown on different soils, should differ from one another?—Is it not rather to be wondered at, that they should agree so nearly as they do? Yet, how often has the correctness of these experiments been called in question where it was almost impossible that the writers could be mistaken!

LXXI. *Some further Observations on the Use of the Colchicum autumnale in Gout.* By Sir E. Home, *Bart., V.P.R.S.*§

I laid before the Society, some experiments and observations in favour of this medicine acting upon the gout through the medium of the circulation, and not by its effects directly upon the stomach and intestinal canal.

The object of the present paper is to show that the infusion throws down a deposit, the separation of which does not appear to diminish the specific effects upon the gout, and renders those upon the stomach and intestines milder than when the deposit is taken along with the infusion.

* Nov. Organ. lib. ii. Aph. 18. † Transport du Bois, p. 460 and 470.
‡ Mem. de l'Acad. Scien. Paris, 1741, p. 328—332.
§ From the Transactions of the Royal Society, 1817, part ii.

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The bulb of the *Colchicum autumnale* contains a certain quantity of extractive matter, and a large portion of mucilage, both of which are taken up by the wine, in the first instance: when the strained liquor is allowed to stand, a considerable deposit almost immediately takes place.

In the first trials that were made with this medicine in St. George's hospital, it was natural to inquire whether this deposit contained any medical virtues; and upon trials frequently repeated, it was found to have none.

This led to the opinion that the extractive matter suspended in the wine, was alone the active part of the medicine; and not only the first deposit was inert, but also that which from time to time was afterwards found to take place.

Of this opinion I was led to entertain considerable doubts, in consequence of having found upon one occasion, in which I took half a bottle of the *Eau Medicinale* which had been poured off without shaking the bottle, that the sensible effects were very mild; those produced by the other half, in which the deposit was mixed, were unusually severe, the nausea being greater, and a greater number of stools being produced.

These doubts were much strengthened, when I found that the effects of the *Eau Medicinale* are more violent upon many stomachs than those of the vinous infusion of the *Colchicum*, which probably arises from the *Eau Medicinale* being kept in small bottles, in consequence of which all the deposit that takes place is given along with the infusion, while the vinous infusion of *Colchicum* being kept in large bottles, the deposit falls to the bottom. If such deposit increased the powers of the medicine in counteracting the symptoms of gout, it would be unnecessary to prosecute this investigation further, since it would be absurd to diminish the violence of a medicine, if, by so doing, its efficacy is to be diminished in an equal degree.

To ascertain this point, I gave sixty drops of the vinous infusion of *Colchicum*, in which there was no deposit whatever, to a man labouring under a severe paroxysm of gout, to which he was a great martyr, and whose paroxysms were usually of several weeks continuance; he was sixty years of age.

The medicine was exhibited on the 17th of January 1817, his pulse being 115. In half an hour he had slight nausea, which soon went off. In five hours, a profuse perspiration came on, and the pain of the gout entirely subsided, leaving a soreness in the parts that had been affected. In twelve hours the bowels were gently moved, his pulse 105 and irregular; in fourteen hours his bowels were acted on a second time; in nineteen hours his pulse was 92, and natural; in forty-eight hours he

was

was quite well, and has continued so a period of more than three months.

The result of this case satisfied me, that the infusion contained the specific remedy for the gout, and that the deposit is not necessary for its removal.

This rendered it probable that, where the deposit is taken along with the infusion, its solid form prevents it from being carried into the circulation of the blood, and it remains in the stomach, producing more or less mischief in that viscus, without being any way concerned in driving away the disease for which the medicine was exhibited; in this respect resembling many of the salts of mercury, which irritate the bowels, without relieving the symptoms of the venereal disease.

I explained these opinions to Mr. Gatcombe, who gives me his assistance in my professional pursuits, and requested him to investigate this subject.

To do this more completely, he began by repeating the three experiments detailed in my former paper, substituting the *Eau Medicinale* for the vinous infusion of *Colchicum*, so as to determine with more precision whether they are or are not the same medicine.

Exp. 1. Thirty drops of the *Eau Medicinale* with the deposit were injected into the jugular vein of a dog: the effects were the same as in my experiment with the same quantity of the vinous infusion of *Colchicum*, only the animal was two hours longer in recovering from them, and was purged for nine hours afterwards.

Exp. 2. Sixty drops of the *Eau Medicinale* were given by the mouth to the same dog: the effect was less than in my experiment with the vinous infusion of *Colchicum* exhibited in the same quantity: this arose from a very copious evacuation of urine having been produced.

Exp. 3. One hundred and sixty drops of the *Eau Medicinale*, injected into the jugular vein of a dog, produced rather more violent effects than in my experiment with the same quantity of vinous infusion of *Colchicum*; the animal died in six hours, and after death the appearances of inflammation in the bowels were more violent, approaching to mortification.

Mr. Gatcombe having found so exact a similarity in the effects of the two medicines, in these trials, I requested him to make the following comparative experiment on the effects produced upon the stomach and bowels by the *Eau Medicinale*, in which there is a deposit, and the vinous infusion of *Colchicum*, in which there is none.

Exp. 4. One hundred and sixty drops of the *Eau Medicinale*, taken

taken by the mouth, produced the same effects, and left the same appearances after death, as when that quantity was injected into the vein, only the animal lived nine instead of six hours.

One hundred and sixty drops of the vinous infusion of *Colchicum* were given to a puppy of the same litter; they produced vomiting, purging, and a great flow of urine; but the animal very soon recovered.

Two hundred drops of the same infusion, after an interval of several days, were given to the same dog, and the effects were the same; the dog had become much improved in his looks and condition.

Three hundred drops, after an interval of several days, were given to the same dog: effects, corresponding with those of one hundred and sixty drops of the *Eau Medicinale*, were produced. The dog died in nine hours, and the appearances of inflammation after death were of the same kind, but not nearly so extensive.

From these experiments the *Eau Medicinale* with the deposit, produces double the irritation on the coats of the stomach and intestines, that is brought on by the vinous infusion of *Colchicum*: this probably arises from the local inflammation brought on by the deposit, upon the internal membrane of these viscera.

To determine as nearly as possible the effects of the deposit, when applied in a solid form to the coats of the stomach and intestines, the following experiment was made.

Exp. 5. Six grains of the deposit of the vinous infusion of *Colchicum* were given to a dog in bread and milk; in three hours it produced vomiting and purging, which lasted twenty-four hours; during the latter part of that time, there was blood in the stools, as well as in what was brought up from the stomach.

I wished to repeat this experiment with the deposit from the *Eau Medicinale*, but found in bottles that had been kept seven years, the wine had become vapid, and, in this decomposed state, the acrid part of the deposit had been taken up again; so that in twelve bottles, containing different quantities, only five grains could be procured, which was quite inert.

Being at a loss to know whether the extractive matter deposited from the infusion is in reality more acrid to the stomach than that suspended in it, or the circumstance of its being applied in a solid form renders it so, I requested Professor Brande to acquaint me, if it could be the effect of any chemical decomposition having taken place.

He favoured me with the following explanation, which is highly satisfactory. "There are certain vegetable bodies which, when infused in water or diluted spirit, furnish a solution which lets fall a sediment, in which their activity, as purgative medicines, chiefly

chiefly resides; this is remarkably the case with the wild cucumber or *Elaterium*. The sediment is a very drastic purge; the part that remains dissolved is comparatively mild in its operation upon the bowels." This explanation of Professor Brande applies to the *Colchicum*, and we are now enabled to separate the purgative qualities of the vinous infusion of *Colchicum* and *Eau Medicinale*, from those which prove a specific for the gout, in the simplest possible manner, by keeping them in large bottles, instead of small ones, and not going too near the bottom.

Is also explains what is asserted by Prosper Alpinus *, that the Egyptian women eat the fresh bulbs, that they may grow fat; an effect which was found to take place in the dog, while the dose was confined within such limits as not to act too violently upon the bowels.

The bulbs of the Egyptian *Colchicum*, when long kept, weigh one drachm each; on being steeped in water they double their weight; so that the quantity of extractive matter contained in two or three recent bulbs, while combined with the mucilaginous matter, of which the bulbs are principally composed, is not likely to be sufficient to do more than act as a brisk purgative, the occasional use of which tends to make people grow fat.

Since this paper was read, the patient who is mentioned as having had the gout in January, has had another attack: it came on the 10th of July, and was removed in the same manner as the former, by the same dose of the medicine. The President of the Society also, convinced by the evidence contained in this and the former paper, that the *Vinum Colchici*, in which there is no deposit, must be a less hurtful medicine than the *Eau Medicinale*, thought it a duty to himself and the public to make trial of it; and on the 20th of July, when the gout in his left hand and the whole of the joints of that side of the body was very severe, allowed me to give him ninety drops of the *Vinum Colchici*, and found that the symptoms of gout were sooner and more completely removed than they ever had been by the *Eau Medicinale*, of which he has an experience of seven years, having taken it regularly ever since the 17th of February 1810, and during that time kept a regular account of the doses, their effects, and the intervals between them.

* Hist. Nat. Egypt. pars 1. lib. 3. cap. 14.

LXXII. *Ex-*

LXXII. *Experiments and Observations upon the State of the Air in the Fever Hospitals of Cork, at a Time when they were crowded with Patients labouring under Febrile Contagion.— By* Edmund Davy, *Esq. Professor of Chemistry, and Secretary to the Cork Institution.*

From numerous experiments made on air collected in different countries by the most enlightened inquirers, it seems to be generally admitted that the chemical constitution of the atmosphere is nearly the same at all seasons of the year and in all parts of the globe. Nitrogen and oxygen gases form its principal component parts; and it also contains a minute portion of carbonic acid gas and a variable quantity of aqueous vapour. As oxygen gas is essential to animal and vegetable life, and to the processes of combustion, fermentation, &c.; and as it is constantly entering into new forms, by which its peculiar properties are modified or destroyed, it is considered the most important and most active part of the atmosphere. The most general and important change that the oxygenous portion of the air undergoes, is its conversion into carbonic acid gas, a substance which, though obnoxious to animals, is yet made subservient to vegetable life; and this change is invariably connected with the exertion of the vital functions of organic beings, and with the burning of coals, wood, candles, &c.

The salubrity and healthy state of the air depend in a great measure upon the quantity of oxygen gas it contains, and this quantity (about twenty-one per cent.) appears to exist in all places exposed to the free atmosphere and the influence of winds. But the same uniformity of composition does not prevail in the air of confined dwelling-houses, crowded theatres, and hospitals that are badly ventilated. At a time when typhus was very prevalent in Cork, and there were in the two Fever Hospitals about two hundred and eighty patients labouring for the most part under febrile infection, it occurred to my friend Doctor Daly, whose active exertions in the cause of humanity are well known, and likewise to myself, that it would be a desirable object to ascertain the state of the air in the fever wards; and I immediately undertook a series of experiments on the subject.

To give in detail all the minutiæ of my experiments would far exceed the limits of this paper; I shall therefore briefly notice my methods and results, and close the communication with a few observations connected with the subject.

I procured air from five large and small wards in the House of Recovery, and from the two wards in Peacock Lane Hospital. I collected it from different parts of the rooms; as in the middle,

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at the sides, near the floor and at different heights from it, and close to the beds of the patients. In every instance, the air was obtained by emptying on the spot bottles that had been previously filled with distilled water, and immediately closing them. The bottles were perfectly air-tight, being all furnished with well ground glass stoppers. The air was examined soon after it had been collected.

The first and most important object of my inquiry was to ascertain the quantity of oxygen gas in the several bottles of air. For this purpose I employed hydrogen gas, and the electric spark, a method that seems to unite more simplicity and elegance than any other, and with due precaution is susceptible of great accuracy. As the purity of the hydrogen used in experiments of this kind is of consequence to the accuracy of the results, it may be proper to notice the mode by which it was obtained, especially as it has, I think, some little novelty, and seems to be quite unexceptionable. I put some small pieces of zinc into a glass, and nearly filled it with water that had been boiling for some time; then filled a tube with the boiling water, and inverted it in the glass; and after adding sulphuric acid, I shortly after collected the gas.

I made a great number of experiments, using in every instance an excess of hydrogen gas. In every trial I mixed 0.30 of a cubic inch of the air under examination, with 0.30 of pure hydrogen gas; and after agitating the mixture in a thick detonating tube furnished with wires, the charge of a Leyden phial was passed through the tube, and the residual air, on being transferred to the cubic inch measure, occupied about 0.40 of it. I venture to state this as a general result; for though in a few cases there was a difference of about 1 per cent. more or less, yet this difference was rather apparent than real, owing to the difficulty of measuring uniform quantities of air, and it was corrected by a careful repetition of the experiments. Now, as two volumes of hydrogen and one of oxygen gas enter into the composition of water; if the foregoing results are made the basis of calculation, the apparent quantity of the oxygen gas in the air from the different fever wards will amount to about 22.22 per cent.—but this is not the real quantity. A slight allowance must be made for a minute portion of air disengaged from the water, after the detonation of the mixed gases; and when this is taken into account, the oxygen may be fairly estimated at about 21 per cent. And according to the statements of Sir Humphry Davy, and other able chemists, 21 per cent. is the actual quantity of oxygen gas in the external atmosphere in different parts of the globe. It may be remarked that the variations in the temperature and pressure of the atmosphere, during the preceding experiments, were

so small as not to influence the accuracy of the general results stated. With a view to confirm the preceding statements, I made comparative trials upon air collected from the open atmosphere at the top of the observatory belonging to the Cork Institution; a situation, perhaps, not less salubrious than any other in Cork.—The experiments were conducted in a manner precisely similar to those I have noticed; part of the same hydrogen was employed, and every precaution used to insure accuracy. And in every case in which the electric spark was passed through a mixture of the air under examination and hydrogen gas, in the proportion of 0.30 of each, the residual air measured about 0.40. I collected air from Hughes's Lane, a place notorious for the number of cases it had furnished of typhus; but it yielded, on examination, the same uniformity of result.

I have made some trials on the other gaseous constituents of the air collected from the different fever wards, and compared them with similar experiments on air from the observatory of the Institution, and I have found a very near coincidence in both series of results. Thus, judging from the absorption that took place in the bottles of air from the fever wards, when placed for some time in water, and when agitated in this fluid, and especially from the effects of lime water on the air; and comparing, by similar trials, air collected from the atmosphere in salubrious situations, I could scarcely, in either case, discover a perceptible difference in the quantity of carbonic acid gas. In one instance, I filled a two-quart ground-stoppered bottle with the air from a large ward at the House of Recovery, and, on the spot, I put into the bottle a small phial of lime water and well closed it.—After much occasional agitation and an interval of about two days, I examined the carbonate of lime formed, and compared it with the quantity produced under similar circumstances from the same bottle filled with air from the Observatory, and treated with lime water: and I was unable in this way to detect any appreciable difference. If this method may be relied on, I think I may venture to state, that the air from the ward did not contain nearly 1 per cent. more of carbonic acid gas than the air from the observatory.

After I had separated oxygen and carbonic acid gas from the different airs examined, I could not detect the presence of any other gas than nitrogen, which exhibited its characteristic negative properties. The want of leisure prevented me from varying and multiplying my experiments, so as to ascertain the exact proportion of the carbonic acid and nitrogen gases in the airs; and it may be proper to observe, that during the time I was engaged in this inquiry, the variations of temperature, moisture, and pressure of the atmosphere were very small, and too often

connected with accidental circumstances to be accurately noticed.

Observations.

Though I did not indulge any sanguine expectations as to the benefit likely to result from a chemical examination of the air in the Fever Hospitals, I thought the inquiry might be useful. If the air in the wards had been found impure, means certainly ought to be adopted in order to improve it; but as this is not the case, the very knowledge of the fact may tend to lull suspicion where it is alive, and create some degree of confidence in the public mind. In this point of view, my investigation may perhaps have some little value, though I am far from attaching any undue importance to it. All my experiments seem to lead to this gratifying conclusion, that there is no material difference in the chemical constitution of the air in the crowded fever wards of this city, and the atmosphere in places that are very generally supposed to be more salubrious. I certainly was not prepared to expect this uniformity of result; but it seems to me to be intimately connected with the *situation*, and more particularly with the *ventilation*, of both Fever Houses. The site (as might be expected) is certainly very good in both cases, and the ventilation, especially in the House of Recovery, seems to be quite unexceptionable.

The necessity of a thorough ventilation in sick chambers, hospitals, &c. is universally felt and acknowledged, and the tendency of this inquiry is to prove its importance. It shows that the air of fever wards crowded with cases of infectious disease may, by a well regulated ventilation, still preserve its salubrity.

Respiration being in all cases a consumption of oxygen or vital air; this process, especially in crowded fever wards, is attended with great loss of oxygen; and a deficiency of this principle is equivalent to an excess of the other two noxious gases, carbonic acid and nitrogen. In circumstances where the uniformity in the composition of the air is every instant destroyed, it is difficult to conceive how it can be momentarily renewed, except by the quick and uninterrupted circulation of its parts. Perhaps, a *thorough ventilation* is, of all others, the most simple, and at the same time the most effectual means of preserving the salubrity of the air in crowded sick wards; and ventilators on the most approved construction, that allow a free ingress and egress of the air, and fires that quicken the circulation, would seem to be the most efficient methods for securing this desirable object.

In close moist weather, and in cases when, from different causes, the air of crowded sick chambers may be damp, or contain an excess of aqueous moisture, the use of quick lime in powder, I presume, will be found very beneficial; it will absorb the excess of moisture, and render the rooms comparatively dry.

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For this purpose, large surfaces of it may be exposed in shallow earthen vessels, in tubs or boxes. The lime will also exert the salubrious effect, of absorbing carbonic acid gas from the air.

Cork Institution, Dec. 6, 1817.

*** To these remarks of Mr. Davy I beg to add, that the results of his experiments tend to establish this truth: that *the matter of contagion* is imponderable, as those substances which, in their state of greatest dilution, merely affect the olfactory organs; but I may also add, like these too, they may be capable of being taken up, neutralized, and precipitated, by chemical agents. Not only effluvia may be added to gases and liquids, but gases to gases, and liquids to liquids, without increasing their apparent volume. It is true that by the instruments of science the mixed gases and liquids may be presented separate; but it would be unphilosophical to infer, because our means cannot yet separate and weigh or measure the matter of contagion, that therefore it has no existence. T.

LXXIII. *Upon the Extent of the Expansion and Contraction of Timber in different Directions relative to the Position of the Medulla of the Tree.* By Thomas Andrew Knight, *Esq. F.R.S. In a Letter addressed to the Right Hon. Sir* Joseph Banks, *Bart. G.C.B.P.R.S.**

My dear Sir,—Many attempts have been made by writers on vegetable physiology, to account for the force with which the sap of trees has been proved by Hale to ascend during the spring, without any hypothesis having been offered which has been thought satisfactory: and almost all which have been offered have been justly rejected as wholly inadequate. I have suggested in the Philosophical Transactions of 1801, second part, page 333, the expansion and contraction of those cellular processes which proceed from the bark to the medulla, which I have there called the true or silver grain of the wood; and which have generally, though most improperly, been called medullary processes. I have there shown, that this substance expands and contracts very considerably under changes of temperature and moisture; and I have stated that a board of oak, which has been formed by cutting across the supposed medullary processes, can scarcely be made, by any means, to retain the same form and position when subjected to various degrees of heat and moisture. I had not at that time ascertained, with accuracy, the comparative expansion and contraction of timber when divided in different directions

* From the Transactions of the Philosophical Society for 1817, part ii.

directions relative to the medulla of the tree, and I was not in possession of any fact which enabled me to prove the existence of any such power, in a state of action, in the living tree. But experiments, which I have made at different subsequent periods, have afforded very satisfactory evidence of the presence of this power in a state of action in living trees, and have also enabled me to ascertain some facts, which appear interesting, and likely to prove useful in directing the proper mode of application of wood for various purposes, in which it is important that it should permanently retain its primary extent and form. These experiments were made upon timber of many different kinds; but as the results were all very nearly the same, I shall confine myself to those made upon the oak, the ash, the beech, and poplar.

Some thin boards of the wood of two of the abovementioned species of trees, the ash and the beech, were cut in opposite directions relative to their medulla, so that the convergent cellular processes crossed the centre of the surfaces of some of them at right angles, and lay parallel with the surfaces of others; by which means I became enabled to mark the comparative extent of their expansion and contraction when they were subjected to various degrees of heat and moisture. Both were placed under perfectly similar circumstances in a warm room, where those which had been formed by cutting across the convergent cellular processes soon changed their form very considerably, the one side becoming hollow, and the other raised; and, in drying, these contracted nearly fourteen per cent. relative to their breadth. The others retained, with very little variation, their primary form, and did not contract more than three and a half per cent. in drying. Both were, subsequently, several times subjected to various degrees of temperature and moisture, and each expanded nearly in the same degree that it had contracted, the form of the one remaining very nearly permanent, and that of the other constantly changing.

A beech and an ash tree, each somewhat exceeding twenty inches in diameter, were felled in the end of January, (at which time the buds of both had become sensibly enlarged,) and a transverse section of about an inch in thickness, and necessarily of a circular form, was immediately cut off from the trunk of each, near its base. An incision was then attempted to be made with a saw from the bark to the medulla, directly in the line of the convergent cellular processes, with the expectation that these, on each side, would expand, and impede the action of the saw. The result was just what I had anticipated, and long before the saw approached near the medulla, it became so strongly compressed that my assistant could scarcely move it. A much thinner saw, which I had in readiness, was then employed; and the incision, which

which was kept open by a wedge, was extended to the medulla. The wedge was then withdrawn, and the opposite sides of the division instantly came in contact with great force. A second incision, similar to the preceding, was then made to commence at the bark, about an inch distant from the preceding, and to terminate, like that, at the medulla; by which means, a wedge of wood, an inch square at the bark, and ending in an edge at the medulla, and ten inches in length, was wholly detached. This, nevertheless, did not quit its position, being retained in it by the expansion of the wood from which it had been separated.

The opposite sides of the same transverse sections of wood were divided by the saw in a direction diametrically opposite to that above mentioned; under which circumstances, the expansion of the convergent cellular processes could not, as in the preceding cases, occasion any pressure upon the sides of the saw, which consequently continued to move with perfect freedom.

These circumstances led me to infer, that the medullary canal must be subject to considerable variations of diameter, with the increase or diminution of the quantity of moisture in the wood; and I conceived that I should easily be able to ascertain the truth or falsehood of this conjecture by the following means. I selected, in winter, some parts of the stems of young trees as soon as they were felled, which I retained in such a situation as might occasion them to lose a considerable part of the water they contained, though not to such an extent as to destroy, or endanger, life. The medulla of these was then removed; and the space it had occupied was filled with cylindrical pieces of metal, which were so large that they could not be introduced without considerable force. The pieces of wood were then deposited in a damp soil, from which they absorbed much moisture; and at the distance of ten days I found the medullary canal so much enlarged, that the pieces of metal dropped through without any pressure being applied.

I am prepared to prove, in a future communication, that the quantity of moisture in the alburnum is subject to great variations in the living tree, and therefore I conclude that the medullary canal frequently changes the extent of its diameter.

It appears probable that, by means of this kind of expansion, the internal parts of timber trees so frequently become rifted or cleft. Winds have been assumed by some, and frost by others, as the cause of these injuries. But winds cannot possibly be the cause, as pollared oak trees, upon which these can exert but very little power, are almost always rifted; and the frost of this climate is rarely, or never, sufficiently intense to congeal the winter sap of trees. This agent must also, I conceive, act suddenly, if it act at all, and the trunks of large oaks cannot suddenly be

be cleft asunder in silence. The oak timber of England is also much more frequently rifted than that of the north of Europe. The force with which the cellular substance of timber expands, is fully equal to produce the preceding effects. I have often seen it overcome the pressure of many tons: it is therefore greatly more than equal to give the impulse to the sap, which was observed by Hale; and as it is obviously in action in the living tree, I must retain the opinion which I formerly gave, that it is the agent by which motion is given to the ascending fluid. How it immediately acts upon the passages through which that fluid ascends, and whether that fluid passes through the cells themselves, or through the intercellular passages described in the elaborate work of Dr. Kieser*, I confess myself to be wholly ignorant; and the slow motion of the fluid, the excessive minuteness of the passages, and the varieties of directions in which it is often moving at one and the same time, will ever render this a question of extremely difficult solution.

There is another kind of contraction in timber whilst drying, and of expansion when subsequently wetted or moistened, which is observable only in lifeless wood; and which has apparently no connexion with the power by which the sap is raised in the living tree. The interior and older layers of wood are much more solid and specifically heavy than the external layers in the same tree; and the latter, consequently, contract more longitudinally in drying than the former, and the edge of every board (that has been cut with surfaces nearly parallel with the line of the convergent cellular processes) which lay nearest the medulla in the tree, will therefore in drying become convex, whilst the opposite edge will become concave. The ill effects of this are often felt when oak timber is employed to form joists, part of these in drying always rising above, and others sinking below the first and proper position. The cause of some musical and other instruments being put out of order by changes of weather, whilst others, apparently similarly constructed, are free from such defects, may probably be traced to one of the sources above mentioned.

I am, my dear Sir, &c.

Downton, April 26, 1817. T. A. Knight.

The Right Hon. Sir Joseph Banks, Bt. G.C.B.P.R.S.

LXXIV. *On the Nautical Almanac for* 1820.

To Mr. Tilloch.

December 20, 1817.

I have just seen the Nautical Almanac for the year 1820; and am happy to find that the attention of the Commissioners of the Board

* Mémoire sur l'Organization des Plantes.

Board of Longitude has been at length turned towards the numerous errors with which that work has lately abounded. I was in hopes, however, that when a reformation had commenced, it would have been complete; and that the Nautical Almanac would have assumed a character and appearance similar to other works of the same kind which are published at Paris and Berlin; and thereby have prevented the necessity of referring to either of those works for information, which is actually the case in the present ephemeris, as we are referred by Mr. Pond, in his preface, to the *Connaissance des Tems* for a catalogue of stars which together with many other tables &c. *ought* to accompany our own publication. As the Commissioners however have not thought proper to enlarge the work, I shall confine my observations to such matters as actually appear in it.

My attention was very soon attracted to the singularity of two prefaces: that of Dr. Maskelyne being ordered by the Commissioners to be retained, *out of respect to his memory*. How the retention of the few lines which he has there written can add any respect to his memory I am at a loss to conceive. They are chiefly historical, and have nothing to do with the present volume. Surely the material part of what he has there stated might have been more properly engrafted in any new preface, and would have prevented that confusion and ambiguity which arise from the two prefaces as they now stand.—For Dr. Maskelyne assures us that the Tables, edited by Mr. Vince, "will be used for the calculations of the Nautical Almanac for *succeeding* years:" whilst Mr. Pond, in his preface, hints at the tables of Burckhardt having been used in computing the place of the moon: but whether such practice commenced in the year 1817, 1818, 1819 or 1820, does not appear quite clear, as he is not so explicit on these points as his illustrious predecessor. It is most probable likewise that Mr. Pond, or Mr. Brown, or *Messieurs* the Commissioners, (for we are wholly at a loss to conjecture under whose management the work is now published,) may think it right to make use of Delambre's *new* tables of Jupiter's satellites; as well as of other tables which have been published since those of Mr. Vince.

But it appears that Mr. Pond was directed to retain the *preface* only; he has therefore retained the whole of the *explanation* at the end, on his own responsibility; and any stranger taking up the work would naturally consider it as the production of Mr. Pond. But, how must the reader be amused at the present day with the description (page 151) which he gives of himself in his voyage to Barbadoes in the year 1763, sitting in Mr. Irving's marine chair, with a telescope from *fifteen* to *twenty feet long*, which he assures us is the *proper* telescope for observing the

eclipses

eclipses of Jupiter's satellites! Surely such incongruities and absurdities ought to be banished from any work which professes to be written for the instruction of mankind.

Mr. Pond, in his preface, makes a feeble attempt to justify the omission of the occultations of the fixed stars; and hints that Dr. Maskelyne was convinced of the little importance of such occultations. But, Dr. Maskelyne has (in the explanation above alluded to) expressly stated that they are inserted in the work in order "to instruct mariners or travellers to look out *frequently* for such observations: which, if they happen to prove occultations and are carefully observed, will afford a *certain* means of determining the longitude of the place of observation." Indeed I believe it will be found that they afford the *best* means of discovering the longitude of any given place: and as such occultations are very numerous (and not *rare*, as Mr. Pond would seem to insinuate) it is to be hoped that the attention of travellers will be drawn towards this branch of the science more than it has hitherto been. In addition to which I would remark that M. Cagnoli has (in the Memoirs of the Italian Society) attempted to show that the true figure of the earth may be ascertained by a connected series of such observations. But, how does it happen (if the Commissioners have given directions that these occultations shall be inserted as formerly) that we find them wholly *omitted* in the present volume? For there is not a single occultation of any fixed star announced throughout the whole of the year: neither are the *conjunctions* of the moon with any of the fixed stars stated, except as to five of the principal ones of the first and second magnitude; viz. β♉, β♊, α♌, α♍, and α♏. It is true that the editor announces an occultation of one of the *planets* (Jupiter) in that year: but why is the occultation of another of the planets (Mars) in January omitted? the true conjunction of which will take place *twenty minutes* later than is stated in the Nautical Almanac. I would observe likewise that the commencement of the solar eclipse in September is set down full *one minute* later than it ought to be; and the point when the moon makes the first impression on the sun's disc ($48\frac{1}{4}°$ from the vertex) is omitted.

Whatever apology might be made for the careless manner in which the late volumes of the Nautical Almanac have been published, arising (as Mr. Pond informs us) from the confusion incident to the death of the Rev. Mr. Hitchins, and the delay attending the necessary instruction of the Rev. Mr. Brown, his successor; yet as this successor has now passed six years of his astronomical education, it was to be presumed that such gross errors and omissions would not have been suffered to disgrace the future volumes of the Nautical Almanac.

I shall

I shall close this long letter, by observing that on casting my eye over the configurations of Jupiter's satellites for the month of January, I find the position of almost all of them to be erroneous. I am, sir,

Your obedient servant,

ASTRONOMICUS.

P. S.—In the preface to the Nautical Almanac it is stated that "*all* the articles were computed by two separate persons, and examined by a third;" perhaps it may exercise the ingenuity and abilities of some of your readers, to determine the *probability* that three persons should commit *precisely the same mistake* in any calculation; and that a repetition of similar errors should occur *several* times in a work of 144 pages!!!.

LXXV. *Prospectus of a new System of Beaconing. By His Majesty's Royal Letters Patent granted to* ROBERT DICKINSON, *Great Queen-Street, London.*

There is something so new in this Prospectus, and the benefits to be expected from the adoption of the system it recommends are so many and so important, that we cannot too earnestly recommend it to the attention and speedy adoption of those who from their situation possess the means of giving efficacy to any plan calculated to benefit the interests of navigation and humanity. Induced by the statements in the Prospectus to examine for ourselves the models of the patentee, we waited on the inventor, and bestowed on them a very careful inspection; and we have no hesitation in stating that, in our opinion, the system is quite scientific, and so perfect as to leave little or nothing to be desired, but its speedy and universal adoption by the common consent and patronage of all the maritime powers.—EDIT.

PROSPECTUS.

The design of this Prospectus is to propose a new beacon, of the following description and uses, with a view to beaconing the seas of the world.

1st. Every beacon on this construction will tell the longitude, latitude, soundings, bearings, and distance from land; how to be approached, currents, &c. with every other particular which the most elaborate and correct survey can describe.

2dly. Every beacon, and the particulars belonging thereto, will be as well known and as familiar to navigators of the remotest climes, and of all nations, as to those of its own country.

3dly. It presents the figure of an erect pillar (see the Plate), and can be placed in all fathomable depths of any reasonable size and elevation, say from 6 to 18 or 20 feet in height.

4thly.

4thly. It will always be found precisely in the spot where it was first laid down.

5thly. Being erect, it can be seen at a much greater distance than the present buoy.

6thly. It will remain completely water-tight.

7thly. While it defies alike the raging tempest, the fields of ice, weeds, the shock of a first-rate man of war, or any other body with which it may be assailed; that of the ordinary size is so reed-like and yielding, that the smallest jolly-boat would not, if suffered to run against it, be in the least injured.

8thly. Lastly, perhaps not least to be regarded, (as it may tend to its being more disseminated over the ocean and different seas,) it can be put down at a small expense; and, incredible as the foregoing may appear, the patentee (after one month's preparation) will engage to furnish twenty beacons a week with all their appendages, and send to any quarter of the globe*.

To show how the superiority now described is effected, the following observations are offered:—It consists, 1st and principally, in the singularity of its shape, which is not very unlike that of a shoulder of mutton before the shank is cut off. 2dly, In the systematic arrangements respecting its moorings; and, 3dly, In its speaking an univeral language.

In giving the bodies intended for sea beacons the form of a cone, (as has always been done,) a great error was committed, as no shape affording so much resistance, and therefore so badly calculated for the passing of the water, could have been found. The next error was, in loading this ill-formed body, which ought to have been as light as possible, with a tremendous heavy chain. Both these evils are here avoided, the shape offered being much sharper in the water than the sharpest Thames wherry; and not being loaded by the chain, as will be shown, the resistance is much less than that of a wherry, and it rides considerably lighter in the water.

The annexed engraving will convey some idea of the improved form given to this beacon; which also, in what regards floatage, presents, it is presumed, a new practical principle, and which, the patentee is vain enough to imagine, will be thought to possess considerable novelty, as hitherto the effect now produced, viz. the floating of a pillar, has never, that he knows of, been accomplished, without the very objectionable incumbrance of an enormous bulk, and a quantity of counterpoising ballast, proportioned to the elevation of the object to be raised. Indeed, it is hardly credible, after the numberless improvements that have been introduced

* Corporate bodies, and such individuals as desire to see the models, with their description, will be pleased to apply by letter, addressed "To the Patentee, 58, Great Queen-street."

troduced into nautical science, that the beacon should have remained, for so many centuries, in a state so defective, seeing as one does (*vide* Lloyd's List) that more casualties and shipwrecks are occasioned by getting aground, (which beacons are intended as, and perhaps are, the only means of preventing,) than from any or all other causes united.—Hence, it is a duty incumbent on mankind generally, to endeavour to render this system perfect, or as nearly so as can be attained by human invention, by human assiduity, and by an accordance of sentiment in all the maritime nations of the world; and, seeing that the benefit to the human race and the advantages of such a union would be reciprocal, it cannot fail, soon or late, of being carried into effect.

The part of the beacon represented out of water, is a pillar, of three or four equal sides, on each of which is painted the same number, whether it be one or one thousand, in such a manner that, when the units are exceeded, the figures must be written downwards: 10 thus, $\begin{matrix}1\\0\end{matrix}$:—15 thus, $\begin{matrix}1\\5\end{matrix}$:—999 thus, $\begin{matrix}9\\9\\9\end{matrix}$:—and, from the form of the pillar, it is difficult to take any position in which the figures will not on one side be seen and distinguished at a considerable distance; and ships beyond reading distance, if they want information, will approach nearer to obtain it.

A Beacon Book, or Formula of References, is to be printed in various languages, wherein will be laid down, by means of corresponding numbers, all the particulars relating to every beacon, and which book, when referred to by the bewildered mariner meeting with a beacon, will, of course, instantly acquaint him with his situation, the dangers and difficulties by which he is surrounded, &c. &c.*

A writer in the American Philosophical Transactions truly remarks, that "the duties of a buoy (meaning a beacon) are *most imperious;* to the performance, however, of which, it is lamentable to reflect, from their construction and appointment, they are wholly incompetent; for, in fact, all they tell is—'Hereabouts is danger;' but on what side, or to what extent, the bewildered stranger is left to guess and find out." And, in truth, it would be difficult for the imagination to conceive an idea of any thing so rude, shapeless, ill-chosen, and unmeaning, as the caun-buoy, the present beacon, as it is called.

* To say any thing respecting the mode of distributing such books, would at present be premature. Those with whom it must lie, to give efficacy to the system, will be at no loss to give them circulation by means of the Custom-houses whence vessels are cleared out for sea.

"But

"But (it may be objected) is it to be expected that all the nations of the earth can be brought to concur in the establishment of such a system?"—To this it may be answered, that, all having an obvious interest in such establishment, it is not unreasonable to believe, that every civilized state may be easily induced to lend its aid to the perfecting of a plan which promises so many benefits to the human race generally. In the mean time, it is consoling to humanity to know, that, among ourselves, there is no want of either heads or hearts to patronize and cherish any rational plan, which has for its object the saving of the lives of thousands of our fellow-creatures (now sacrificed to a system left defective, merely because the possibility of a remedy was not contemplated), and adding much to the comfort and happiness of all who are doomed to traverse the ocean. The Right Honourable the Lords of the Admiralty, the Minister for the Foreign Department, the Brethren of the Trinity House, are sufficient to call it into action without any foreign concurrence. On our own coasts there is much occasion for it; nor can it be reasonably doubted, that, meeting with the countenance of our own Government, most of the European maritime powers, and also the United States, would easily be induced to lend a hearty co-operation.

In favour of any exertion that may be made for establishing a general system of Beaconing; it is to be remarked, that the contrivance already alluded to, of employing buoys attached to different parts of a chain (see the engraving), to act as *carriers*, besides furnishing a means for planting beacons in comparatively deep seas, is calculated to promote the undertaking by the facilities which it affords in point of expense. The chain, as already stated, may be very small; for *each carrier bears its own portion of it*, and the ultimate strength wanted is only what may be required to withstand the current (when there is one) and the wind; neither of which can ever exercise any power upon the beacon, at all to be compared with what is now required to sustain a common beacon chain *. The beacon itself has nothing to carry but a few links of that portion by which it is united to the upper *carrier*; and from its form, and the material of which it is made, (viz. metal †,) suffers any vessel or other floating body

* The strength required when only the strain occasioned by wind and current is to be provided against, is much less than most people would imagine. In an experiment made at sea, off Southend, in twelve-fathom water in a very high wind, a piece of common jack-chain (unable to sustain two hundred weight without breaking) was found perfectly adequate to keep a beacon exposing six feet of height above the surface, in its place, the chain being borne by three *carriers*.

† Experience has shown that wood, as a material, is but ill adapted for marine

body which may come in contact with it, to pass, without any other effect than moving it to one side, or passing over it; after which, it will instantly recover its position, and perform its duty as before; so that the expense of maintenance will be trifling. Nor is the saving in weight (which in every case will be at least 80 per cent.) the only benefit that results from the use of *carriers*: the greater part of the expense of manufacture can also be dispensed with, straight rods linked to each other at their ends, answering as well as the most expensive chains.

The advantages which will present themselves to the minds of those acquainted with nautical matters, as likely to result from this system, must be manifold beyond any thing that the author (who is no sailor) can conceive; but one thing is obvious, that it must prove highly beneficial that these beacons (instead of rolling about like so many porpoises, scarcely visible,) are always standing erect, exhibiting a height of from six to twenty feet above the surface, and may be seen to intercept the line of the horizon at several miles distance.

Nor is the proposed system applicable to shallows only. As it provides means for sustaining chains of any length, it is now possible to plant beacons in any seas that can be sounded. And it deserves particular notice, that the method which has been devised for sustaining chains, however heavy, proves at the same time, a means for rendering chains that are comparatively light, able alike now to perform all that duty which formerly required very heavy and strong chains. Nay, more: light chains can now be made to perform what could not be done at all formerly; for in proportion to the depth, so it was then necessary to increase the strength, not merely to enable the chain to restrain the buoy, unnecessarily bulky, &c. and improperly loaded, but even to sustain its own weight *. From this circumstance, the utmost depth that could be

marine beacons. It is apt to admit water, and need tapping, easily damaged by worms, subject to rapid decay, and but ill suited to be worked into the best form for a beacon. The patentee has adopted iron, as a material subject to none of these objections, being homogeneous, impervious to water and worms, and expanding or contracting equably in all its parts, when exposed to changes of temperature. Should it be objected that iron will soon be destroyed by rust, it is answered, The patentee has a method of coating his iron, so as to defend it for a great number of years; as is proved by some beacons furnished by him for Government, and which have been for a considerable time in use at the Island of Bermuda.

* The common beacon, having a great weight to carry, is necessarily obliged to be made very bulky; and in consequence, there is a constant struggle between the buoy and the chain at the passing of every wave; by which repeated tugging action, the block to which the other end of the chain is made fast, is, by innumerable and constantly repeated hitches, gradually removed from its place, sometimes a mile or two; an event that never

be reached was, comparatively, very limited, no means being known before, whereby it could be accomplished. The thing, however, is now practicable; and, sooner or later, it will be effected; for it is equally rational that the seas should be furnished with navigation-posts, as that travellers by land should have the convenience of mile-stones and finger-posts provided for them.

THE STAR *Newspaper, of the 29th of September* 1817, *contains the following Extract of a Letter from Derry.*

"SHIPWRECK.

"Ferry Side, Carmarthen, September 26th, 4 P.M.

"I am sorry to inform you, that at about four miles distance, a brig with yellow sides, and about two hundred tons burthen, has got on a point of sand: her masts are gone overboard, and she must be a total loss, as the wind is strong from the S.W. with a heavy sea:—a boat is observed full of men, going into Kidwelly; but whether it is the crew of the vessel, or some men intending to go to her assistance, we are unable to ascertain.

"Seven o'clock, P.M. The tide is now on the turn, not a vestige of the vessel is to be seen, she totally disappeared about an hour ago, and our opinion is, that the captain is a stranger on our coast."

It does not follow, because the present system is of great antiquity, enormously expensive, and has been got up with wonderful and praise-worthy labour and attention, that it is complete; nay, that it is not most lamentably defective, as, indeed, we know it to be, from the melancholy accidents arising almost daily from the single circumstance of getting aground:—The pains which have been taken, only prove that the necessity for doing something was so urgent, that something must be done, and the best has been done that happened to be thought of. Nevertheless, were this system under the exclusive control of one individual, it would not be surprising to find him clinging to it, for no other reason but because it was old, laboured, and expensive; but under the enlightened management of a corporate body, like that of the Trinity House, composed as it is of talent and respectability, which true merit alone can influence, defeat and delay on this occasion are not to be apprehended. The present proposal

never can occur with the telegraphic or pillar beacon, which having no belly above the water line, is not affected by the waves, and having only its own fastening to carry, requires from the block and its chain no more than simply to resist the current; a pressure to which the strength of one man is more than equal. The consequence of a beacon changing its situation is, that it changes also its character, and instead of being the mariner's beacon and friend, becomes a deceiver, and a decoy to his destruction.

* The summits of the new beacon are made conical and sharp pointed, to prevent birds from resting on them, and obliterating the figures.

will

will no doubt be [illegible] examined; and, if found worthy, adopted. The expense will not then be thrown away upon a system comparatively worthless, and which has been submitted to, only for want of possessing better means: and when the system now recommended shall have become general, there will be no longer "strange captains on any coast:" and also it should be considered, that as the light expense at which it can be carried into effect will allow an increase of beacons, (it is thought of nearly ten to one,) the security will be increased in a tenfold ratio, independent of the duty being so much better performed.

LXXVI. *Notices respecting New Books.*

The Second Part of the Philosophical Transactions of the Royal Society of London, for 1817, has just been published, and contains the following papers:

XIII. Description of a thermometrical Barometer for measuring Altitudes. By the Rev. Francis John Hyde Wollaston, B.D. F.R.S.—XIV. Observations on the Analogy which subsists between the Calculus of Functions and other Branches of Analysis. By Charles Babbage, Esq. M.A. F.R.S.—XV. Of the Construction of Logarithmic Tables. By Thomas Knight, Esq. Communicated by Taylor Combe, Esq. Sec. R.S.—XVI. Two general Propositions in the Method of Differences. By Thomas Knight, Esq. Communicated by Taylor Combe, Esq. Sec. R.S. —XVII. Note respecting the Demonstration of the binomial Theorem inserted in the last Volume of the Philosophical Transactions. By Thomas Knight, Esq. Communicated by Taylor Combe, Esq. Sec. R.S.—XVIII. On the Passage of the Ovum from the Ovarium to the Uterus in Women. By Sir Everard Home, Bart. V.P.R.S.—XIX. Some further Observations on the Use of Colchicum autumnale in Gout. By Sir Everard Home, Bart. V.P.R.S.—XX. Upon the Extent of the Expansion and Contraction of Timber in different Directions relative to the Position of the Medulla of the Tree. By Thomas Andrew Knight, Esq. F.R.S. In a Letter addressed to the Right Hon. Sir Joseph Banks, Bart. G.C.B. P.R.S.—XXI. Observations on the Temperature of the Ocean and Atmosphere, and on the Density of Sea-water, made during a Voyage to Ceylon. In a Letter to Sir Humphry Davy, LL.D. F.R.S. By John Davy, M.D. F.R.S. —XXII. Observations on the Genus Ocythoë of Rafinesque, with a Description of a new Species. By William Elford Leach, M.D. F.R.S.—XXIII. The distinguishing Characters between the Ova of the Sepia, and those of the Vermes testacea, that live in Water, explained. By Sir Everard Home, Bart. V.P.R.S.—XXIV. Astronomical Observations and Experiments tending to investigate the local Arrangement of the celestial Bodies in Space, and

and to determine the Extent and Condition of the Milky Way By Sir Wm. Herschel, Knt. Guelp. LL.D. F.R.S.—XXV. So an Account of the Nests of the Java Swallow, and of the Glands that secrete the Mucus of which they are composed. By Sir Everard Home, Bart. V.P.R.S — XXVI. Observations on the Hirudo complanata and Hirudo stagnalis, now formed into a distinct Genus under the Name *Glossopora*. By Dr. Johnson, of Bri[illegible] Communicated by Sir Ev. Home, Bart. V.P.R.S.—XXVII. Observations on the Gastric Glands of the Human Stomach, and the Contraction which takes place in that Viscus. By Sir Everard Home, Bart. V.P.R.S.—XXVIII. On the Parallax of the fixed Stars. By John Pond, Esq. Astronomer Royal.

Sui Cangiamenti di Colore della Tintura del Turnesoli, &c. *Observations and Experiments on the Changes of Colour in Tincture of Turnsole, and other Vegetable Tinctures. By Professor* Branchi *of Pisa.* 8vo. 1816. pp. 112.

The universal use of tincture of turnsole as a test, seems to have induced the universal belief that it was good and sufficient. Our Pisa Professor, who always ventures to think and observe for himself, discovered the fallacy of this notion, and on investigation found that very little was known on that subject, and that the opinions which have been promulgated respecting it are remarkable for nothing but their contradictoriness and absurdity. Turnsole being a manufactured substance in which *Lichen Perillus*, *Croton tinctorium*, *Variolaria orcina*, or other Lichens, may form the basis, it is not extraordinary that its chemical elements should be different. Chevreul * found it consisted of colouring matter, of muriate, sulphate, and subcarbonate of potash; of carbonate of lime; of alumina and oxide of iron and of silica. The author analysed three different kinds, and found nearly the same results except the muriate of potash, of which he only found some traces Of 288 grs. of turnsole of three different qualities, the first yielded a residuum insoluble in water 226½ grs.; the second 163¾, and the third 184½ grs. Of a residuum insoluble in acetic acid, the first gave 209½, the second 134, and the third 151¼ grs. Of a residuum insoluble in muriatic acid, the first left 202, the second 123½, and the third 132¾ grs. But it appears that in turnsole of the same quality there is not always the same quantity of insoluble matter, and that the colouring matter is soluble in alcohol in the inverse ratio of its strength: when dissolved however in strong alcohol, by refracted light it had a blue colour, more or less violet; with reflected it was red. The Professor proceeds to collate all the opinions of the different chemists who have treated of turnsole, and brings them to the test of experiment, in which he evinces equal ingenuity and address.

* Ann. de Chimie, 88.

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The idea of C[illegible], that the colouring matter of turnsole is the result of a colouring principle being united to an acid, is experimentally disproved; but the opinion most remarkable for self-contradiction and more than usual absurdity, is that quoted from the French translation of Dr. T. Thomson's Chemistry. In it the author confounds tincture of turnsole and syrup of violets, and says that the acids change vegetable blue colours into red; but that, if these colours have been rendered green by the alkalies, the acids make them re-appear and restore them. Turnsole is not changed into green by alkali, and even the restoring of the colour to syrup of violets must depend on a very exact saturation. The tincture of turnsole, it appears, spontaneously changes its colour from blue to yellow, and then blue again, whether exposed to or excluded from the air, and at the same time some sulphuretted hydrogen gas is evolved. These spontaneous changes of colour take place in the course of a few days; subcarbonate of potash or alcohol, added to the tincture, will prevent it from changing its colour for two years The change into yellow is attributed to the sulphuretted hydrogen, which is derived from the decomposition either of the vegetable or animal matter, urine being used for the preparation of turnsole. The final result is, that the tincture of turnsole is subject to change its colour and become yellowish in more or less time; that it does not always experience this alteration more rapidly in consequence of being prepared with hot water; that it loses its colour oftener when entirely excluded from the air than when partially exposed; that an alkaline solution of carbonate of potash in a sufficient dose prevents it from losing its colour, and that alcohol has the same effect; that being reddened by acid and kept in a close vessel, it suffers no further change; that it is discoloured with a little acid and takes the colour of red wine, which finally becomes blue on exposure to the air or to ebullition; that by this means it is more capable of indicating the existence of an acid in a small quantity; that the red vinous colour is owing to carbonic acid; that by means of phosphorus it becomes red on exposure to the atmosphere; that when exposed to the solar rays it undergoes much greater changes in open than in close vessels; that in repeated changes of colour it precipitates some flakes of insoluble matter; that when its colouring matter is almost entirely decomposed in a close vessel, it has then experienced the greatest number of discolorations; that on becoming yellow in a vessel containing atmospheric air, it abandons part of its carbon, which with the oxygen of the atmosphere and caloric forms carbonic acid gas; that the alkalised or acidulated tinctures do not sensibly alter the air with which they are in contact; that the discoloured tincture has sometimes the smell of sulphuretted hydrogen gas, which is manifested by paper moistened with a solu-

tion of acetate of lead, or by a piece of silver; and lastly, that this sulphuretted hydrogen seems owing to the spontaneous discolourment of the tincture itself.

The Professor proceeds to examine the changes of colour in the tinctures of orchella, violet, Brazil and Campeachy woods, &c. The orchella owes its name to a native of Florence, its reputed discoverer about the beginning of the 14th century, called Oricellaria or Rucellai; but, like most of the Florentine arts, is most probably of Greek origin. Both the aqueous and spiritous tinctures of orchella manifested the same changes and characters as the tincture of turnsole. The orchella of commerce is of two kinds; one called "vegetable or Canary orchella," which is prepared by fermenting the *Lichen rocella* with urine, and the other "earthy orchella," prepared from the *Lichen Parillus* of Auvergne. The aqueous tincture changes colour more rapidly than the spiritous; but the other results are similar, and prove that the colouring principle of turnsole and orchella is the same as alleged by Chaptal, who learned the fact from an English tract on colours, by Wilson, published about half a century ago, a work almost unknown in England at present, but which has been deeply studied by Chaptal and other French chemists, and in which will be found the germ of many of their professed modern discoveries. The tincture or infusion of violets, sometimes called syrup, because the latter was formerly used in medicine, changed from blue to purple or violet, at the same time it evolved carbonic acid gas, and regained its colour by the addition of a very small quantity of a solution of common potash or tin. The infusion of violets may therefore be kept in a tin vessel several years, and be still fit for chemical purposes. The colouring matter in the roots of *Anchusa tinctoria* L. is soluble in alcohol and in oil, but not in water. A strong spiritous tincture kept seven months in an air-tight bottle without undergoing any alteration. Similar and even still more extensive experiments were performed on the colouring matter of Brazil or Pernambuco wood (*Cæsalpinia echinata*), Brasiletto (*Cæsalpinia Sapan*) or wood of St. Martha, Campeachy wood (*Hæmatoxylon Campechianum*), and on nephritic wood, the *Moringa Zeylanica*. The aqueous infusion of the last is celebrated by Newton for having the property of refracting the yellow or orange rays of light and reflecting the blue, of becoming yellow even with reflected light by means of an acid, and of re-assuming the blue by the addition of an alkali. The author has found that several varieties of ebony, sandal, and iron wood have the same properties, and his observations on guaiacum confirm those of Mr. Brande. It appears that a little Brazil-wood sawdust mixed with some natron or impure carbonate of soda, and put into a tumbler of water, immediately communicates to it the colour and appearance of red wine; and that when

this

this coloured fluid be poured into another glass containing a few drops of lemon-juice, it instantly becomes like white-wine, or colourless. The Italian mountebanks have used their knowledge of this fact to delude the vulgar or the ignorant. Professor Branchi relates many other curious phenomena, and refutes many generally received erroneous opinions respecting the colouring matter of these woods. But of a work which consists entirely of facts and original experiments, without any admixture of fine-spun theories, it is impossible to convey any adequate or just idea by means of a brief analysis: we must therefore be content here to close the notice we have been induced to take of this highly ingenious and scientific work.

An Essay on Electricity, by Ferdinand Elice, *Doctor in Philosophy and Medicine, Member of the College of Philosophy and Polite Literature, formerly Assistant Professor of Experimental Philosophy in the University of Genoa, Rector and Professor of Philosophy in the College of Cava, &c.*—Genoa 1817.

This is a very clear and comprehensive view of the history and principal phænomena of electricity and galvanism—a work which must be of great utility in Italy, where books of science are very scarce and dear, where communication with men of science is difficult, and where the votaries of pleasure greatly preponderate over the few and comparatively obscure admirers of experimental philosophy, physical truth, or the phænomena of nature. It is not indeed to be expected, that any branch of human knowledge which requires the exercise exclusively of the rational faculties can flourish in a country devoted solely to sensual enjoyments, to the slavery of fashion, and to a puerile luxury in dress more characteristic of people just emerging from barbarism, than of those who have reached the noontide of civilization. We need not therefore be surprised that all chemical and other scientific knowledge is confined to the Professors of the colleges; and it must always remain so when a ballad-singer and a fiddler can find 300 auditors to attend their lectures, while at the lectures of an enlightened chemist there are only three ragged boys.—Professor Elice however has been more fortunate, and has been honoured by the attention of some persons of rank: he has therefore pursued his researches with more spirit; and from the sum of his labours he has extracted this Compendium, and systematically condensed much varied information on electricity into 70 pages. He adopts in general the Franklinian theory, and admits with candour some of its insuperable difficulties. In speaking of the identity of effect which both positive and negative electricity have on the animal œconomy, he confesses his inability to conceive (or to reconcile this hypothesis with the phænomena)

how the total want or the diminution of a [illegible] should produce the same effect as its accumulation. [illegible] this is not the only instance where sameness of effect is apparently produced by opposite or different causes in the animal economy, and it originates, perhaps, in our very limited knowledge of the latter. Other phænomena are more difficult to explain, such as the permeability of glass by the electric fluid, &c. The author, in common with most of the electricians in France, seems to have known something of the discoveries of the Rev. Mr. Lyon of Dover, whose writings and researches on electrical phænomena from 1767 to 1807 are much better known on the continent than they are in England. It is an error of some original writers to despise too much contemporary fame, in consequence of which their discoveries are appropriated by others; and foreigners often avail themselves of this circumstance to profit by their labours without acknowledgement. In justice to the Genoa Professor it may be observed, that he has followed the French; and that he is extremely careful, in quoting authorities, (such as he finds them in French translations of English books,) in assigning to each his true merit and portion of discovery, and in tracing the progress of knowledge respecting this still half known science.

Dr. Crichton has just published An Account of some Experiments made with the Vapour of Boiling Tar in the Cure of Pulmonary Consumption.

Mr. Accum has in the press a second Edition of his Chemical Amusement, comprising 160 curious and instructive experiments in chemistry, which may be performed with safety in the closet, and the exhibition of which does not require the aid of costly and complicated instruments. The work will be illustrated with plates engraved by Lowry.

The First Part of Mr. William Smith's Stratigraphical System of Organized Fossils, with reference to the Geological Collection deposited in the British Museum, showing their use in identifying the British Strata, has just made its appearance, price 15*s*. To simplify and elucidate Geology, by exciting the attention of the curious to numberless new objects in nature, which may call forth the industry, talent, and capital of others, to explore and extract the subterraneous wealth of the country, when the employment of the people is an object of legislative inquiry, must be nationally useful; nor can any one doubt the utility of such minute researches, who considers that his food, his clothing, and every article around him, comes directly or indirectly from the soil.—Part II. which completes Mr. Smith's work on this branch of Geology, will be speedily published.

The

The Eighth Number (which completes the second volume) of The Memoirs of the Caledonian Horticultural Society has just been published. Contents:

History.—Discourse by Dr Duncan senior, read 4th December 1815—Discourse by the same, read 3d December 1816. Memoirs.—Report of the Committee for Experiments on the Naturalization of Useful and Ornamental Plants under the Climate of Scotland; with Prefatory Remarks on the Character and Economical Uses of Forest Trees, already introduced, and an Enumeration of certain Exotic Plants which have lately withstood the Winter of North Britain; drawn up by John Yule, M.D.F.R.S.E.—Mr. Wales's Account of an easy and sure Method of raising Mushrooms, either with Dung or without it.

Dr. MacLean has just published a very interesting work on Epidemic and Pestilential Diseases; the principal doctrine of which is, that the cause of epidemics resides in the qualities of the air; in this respect it is correspondent to the doctrine of a small tract just published by Mr. Thomas Forster, on Atmospherical Diseases. Dr. MacLean, however, has entered very minutely into the erroneousness of the general practice in these kind of disorders, and has shown in a masterly manner, that the practice now prevalent is not only useless, but is the principal cause why such a number of people are every year carried off by the plague and other epidemics. One of the most curious facts mentioned by Dr. MacLean is, that the doctrine concerning *contagion* owes its origin to a Catholic stratagem in the middle of the sixteenth century, whereby an endeavour was made by the Legates of Pope Paul III. to remove the Council of Trent to Bologna, by spreading a report that an epidemic disease then prevalent at Trent was infectious. His observations on quarantine are highly deserving the notice of all who have the direction of medical police.

A Synoptical Catalogue of British Birds has been published by Messrs. Nicholls and Co. intended to identify the species spoken of by different provincial names in various counties of Great Britain. It contains also the valuable additions and generic arrangement of Dr. Leach, from a Catalogue he recently printed.

Mr. Abernethy has just published his Third Course of Lectures at the London College of Surgeons, on Mr. Hunter's Theory of Life, and on his Museum. It appears that many of the most important discoveries, assumed by recent physiologists have been plagiarized from that celebrated surgeon.

Mr. W. J. Hooker and Dr. Taylor have just published a work on the Mosses of Great Britain and Ireland, entitled "Muscologia

logia Britannica," which contains figures and descriptions of each Species native of these islands; together with plates illustrative of the Genera. 8vo.

Mr. Hooker has likewise published the first Number of a work on the new and rare or little-known exotic Cryptogamic Plants; with which will be incorporated those collected in South America by Messrs. Humboldt and Bonpland, and various other interesting subjects in the possession of the author and his botanical friends. This will have numerous plates, and appear in an 8vo form.

LXXVII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

St. Andrew's day falling this year on a Sunday, the Royal Society held their annual meeting December 1st at their apartments in Somerset Place, when the President the Right Hon. Sir Joseph Banks, Bart. G.C.B., after a very able speech on the Determination of an Invariable Standard of Linear Measure, presented, in the name of the Society, the gold medal called the Sir Godfrey Copley's medal, to Captain Henry Kater, for his Experiments for determining the Length of the Pendulum vibrating Seconds in the Latitude of London. The Society afterwards proceeded to the choice of a Council and Officers for the year ensuing; when, on examining the lists, it appeared that the following gentlemen were elected:

Of the Old Council.

The Right Hon. Sir Joseph Banks, Bart. G.C.B.—William Thomas Brande, Esq.—Samuel Goodenough, Lord Bishop of Carlisle.—Taylor Combe, Esq.—Sir Humphry Davy, Knt. LL.D. Sir Everard Home, Bart.—Samuel Lysons, Esq.—George, Earl of Morton.—John Pond, Esq. Astronomer Royal.—William Hyde Wollaston, M.D.—Thomas Young, M.D.

New Council.

George, Earl of Aberdeen.—Davies Gilbert, Esq. M.P.—Charles Hatchett, Esq.—Capt. Henry Kater.—William Howley, Lord Bishop of London.—Right Hon. Charles Long, M.P.—John Reeves, Esq.—Richard Anthony Salisbury, Esq.—Edward Adolphus, Duke of Somerset.—Glocester Wilson, Esq.

Officers.

President, Right Hon. Sir Joseph Banks, Bart. G.C.B.

Treasurer—Samuel Lysons, Esq.

Secretaries—{William Thomas Brande, Esq.
Taylor Combe, Esq.}

After the election, the members dined together as usual at the Crown and Anchor Tavern in the Strand.

ROYAL

ROYAL SOCIETY OF EDINBURGH.

Monday, 15th December, a paper was read before the Royal Society of Edinburgh, which had been announced at the first meeting in November, by Dr. Murray, containing Experiments on Muriatic Acid. He had repeated the experiment performed by Dr. Ure, of subliming muriate of ammonia over ignited metals, with the variation of operating on the salt formed by the combination of muriatic acid and ammoniacal gases, instead of the common sal ammoniac, which from its mode of preparation might be supposed to contain water. He obtained a similar result, water appearing when the muriate of ammonia was sublimed over iron at a red heat in a glass tube. His attention having been thus recalled to the subject, he repeated the experiment which he had performed some years ago, of obtaining water from muriate of ammonia by heat, employing an apparatus somewhat on the principle of Dr. Wollaston's Cryophorus, and with a successful result. He then submitted muriatic acid gas to experiment in various modes. Iron filings perfectly dry and clean having been put into a glass tube, surrounded with sand, and placed across a furnace, so as to be raised to a red heat, muriatic acid gas extricated from a mixture of supersulphate of potash and muriate of soda, and conveyed through a tube containing dry muriate of lime adapted to the other, was transmitted over the ignited iron. Moisture immediately appeared in the tube beyond the ignited space, and soon collected in globules, and hydrogen gas was disengaged. In another experiment the gas was previously kept in contact with muriate of lime for a number of hours, and was then passed from the jar over the ignited metal with a similar result. And in another form of apparatus, still better adapted to afford a perfect result, and to obviate any fallacy from the presence of aqueous vapour, muriatic acid gas was conveyed, from a jar in which it had been exposed to dry muriate of lime, through a bent tube, into a tubulated retort containing dry zinc filings; heat was applied by a lamp to favour the action of the metal on the gas: moisture condensed in the curvature and tube of the retort, and hydrogen gas was collected at the extremity, which terminated under mercury. The heat was renewed at intervals for three or four days, with the requisite addition of fresh quantities of the muriatic acid gas, and the production of moisture increased, until a very sensible quantity of water was obtained at the end of the experiment. The prosecution of the subject is announced in a continuation of the paper to be read at a future meeting of the Society.

LXXVIII. In-

LXXVIII. *Intelligence and Miscellaneous Articles.*

SAFETY-LAMP CONTROVERSY.

In our last Number we laid before our readers the Resolutions of Mr. Stephenson's friends, held at Newcastle on the 1st of November; also the Resolutions of a Meeting held at the house of Sir Joseph Banks, on the 20th of the same month, for considering the facts relating to the discovery of the Lamp of Safety. Since that time we have received a "Report upon the claims of Mr. George Stephenson relative to the invention of his safety lamp," published by his Committee, to which are prefixed the Resolutions first alluded to above.

At present we can only find room to notice in this Report a palpable case of piracy elicited by the questions as put by Mr. Stephenson's own Committee. Mr. Stephenson admits his having published sketches of lamps different from those he really tried, and having adopted the safety-screw and trimmer of Sir H. Davy. His friends did not venture to ask him whether he had not also given a texture of metal the same as Sir Humphry's gauze to his chimney and air-feeder.

In our next we shall lay before our readers some further observations on the groundless claims of Mr. Stephenson. In the mean time we submit to their inspection the proceedings of another meeting held at Newcastle since Mr. Stephenson's friends published their Resolutions.

"*Assembly Rooms,*

"Newcastle, Nov. 26, 1817.

"At a General Meeting of the Coal-Owners of the Tyne and Wear, convened 'for the purpose of taking into consideration certain Resolutions passed at a Meeting of the Friends of Mr. George Stephenson, on the 1st inst. the tendency of which impugns the justice and propriety of the proceedings adopted at a Meeting of the Coal Trade on the 31st August 1816,'

"John George Lambton, Esq. M.P. in the Chair:

"*It was resolved*,—That this Meeting feel themselves called upon, as an act of justice to the character of their great and disinterested benefactor Sir Humphry Davy, and as a proof that the Coal-trade of the North in no way sanctions the Resolutions of Mr. Stephenson's friends on the 1st November 1817, to state their decided conviction, that the merit of having discovered the fact that explosions of fire-damp will not pass through tubes and apertures of small dimensions, and of having applied that principle to the construction of a safety-lamp, belongs to Sir H. Davy alone.

"That this Meeting is also decidedly of opinion, from the evidence

dence produced i[illegible] publications by Mr. George Stephenson and his frie[illegible]ntly to the meeting of the Coal-trade, which was held [illegible] of March 1816, and from the documents which have been read at this meeting, that Mr Stephenson did not discover the fact that explosions of fire-damp will not pass through tubes and apertures of small dimensions, and did not apply that principle to the construction of a safety-lamp; and that the latest lamps made by Mr. Stephenson are evident imitations of the lamps of Sir H. Davy, and even with that advantage are so imperfectly made as to be actually unsafe.

"That the Resolutions now passed be published thrice in the London, Newcastle and Durham papers, and in the Edinburgh Courant; and that printed copies thereof be sent to the Lords Lieutenants of the two counties, the Lord Bishop of Durham, and the principal owners and lessors of collieries upon the Tyne and Wear.

"J. G. Lambton, Chairman.

"The chairman having left the chair, the thanks of the meeting were unanimously voted to him for his able and proper conduct in the chair."

STEAM ENGINES IN CORNWALL.

The following were the respective quantities of water lifted one foot high with one bushel of coals by thirty-four engines, reported by Messrs. Lean in the month of November.

		Pounds of water	*Load per square inch in cylinder.*
26 common engines averaged		21,290,401	various.
Woolf's at Wheal Vor	..	34,376,633	19·4 lib.
Ditto Wh. Abraham	..	34,251,269	10·9
Ditto ditto ..	..	41,289,317	16·8
Ditto ditto ..	..	25,220,603	4·3
United Mines engine	..	35,247,309	18·1
Treskirby ditto ..	..	34,169,262	10·4
Wheal Unity (Woolf's)	..	34,323,944	13·1

To Mr. Tilloch.

Sir,—My visit to the Isle of Man has precluded me the pleasure of perusing the latter numbers of your journal; this circumstance must be my apology for not earlier noticing some remarks preferred on the opinion I advanced relative to the results of the functions of vegetable being. Absence from home prevents any reference to my authorities, nor in my estimation is any thing like this called for, in a particular manner *now*, seeing there is nothing *new* elicited, as has been very properly observed by your correspondent.

If my memory does not much deceive me, Sir Humphry Davy has

has always inclined to the opinion I have advocated—to me no longer a problem. The air from the coast of Guinea was, I believe, submitted to *Dr. Beddoes*, not to Sir H. Davy. The quotation from that able naturalist Brisseau Mirbel stands recommended by its own merits, and demands no eulogy from me. It would have been more wise and becoming to have treated it with the respect it was so well entitled to receive. The compensation provided by this transportation finds a thousand beautiful analogies amid the harmonies of nature. The witticism which wields a term employed by myself calls for no reply,—the expression "floods of oxygen" I see no cause to change. There are many incongruities obtaining in experiments of this nature, arising, it may be, either from the unnatural position of the plant secluded from the influences which minister to the healthy forms of this curious organization, or the imperfect and faulty eudiometrical test. It requires no mean judgement to wield the (*e. g.* the nitrous gas) test, so as to balance the results aright. The experiments hinted at, on one occasion, by Mr. Brande, would seem to intimate that some plants produce the same effects on atmospheric air as animals, while others exhibit no alteration, and a third class refine the medium. It is impossible to comprehend these anomalies, and I have merely adverted to this gentleman's results, in order to show that inaccuracy must exist somewhere. I have no right to assume, far less to decide. On a future occasion I shall renew the subject: meantime, in answer to the position that plants effect the same change upon the atmosphere as animals, I may observe that ANIMALS *constantly* and *without any intermission* enhance a noxious atmosphere, while PLANTS emit the destructive gas *only* at night, when, succumbing by its native weight, being cooled by the dews of even, it sinks harmless on the bosom of the earth. But as a conclusive set-off, meantime, to these *unnatural* experiments on *imprisoned* vegetation, I have to adduce testimony of the highest authority,—that from a series of repeated experiments made at Madras, it was invariably found, that the LAND breeze contained FIVE per cent. more oxygen than the SEA breeze.

You have, Sir, added a note to the appendage of the intercepting partition of wire gauze; but you forgot for the moment, the change of character exhibited by flame when so bisected, as deduced from the experiments of Sir H. Davy and Mr. G. O. Sym, and which I have varied in results of my own:—*extension of surface* might be a better expression of my meaning. It may still, indeed, be doubted whether the term *cooling* is appropriately applied, notwithstanding the highly ingenious and multiplied experiments of Sir H. Davy. I would not be deemed a plagiarist, and in justice to myself must assume the *priority* of attaching

attaching the ca[illegible]tinum wire to the wick. It would appear, though th[illegible]y might at first seem to be the case, that suspension *from the roof* of the *cylinder* was previously contemplated by this eminent philosopher: in proof of this, please be referred to page 231 of your September Number. The safety lamps provided in this manner by Mr. Newman for others and myself prove the same thing; and I have reason to believe that those forwarded by Sir H. himself were similarly constructed. This remark has been elicited by the plate illustrating Sir H. Davy's interesting paper on flame, inserted in a recent number of your journal. Unless the platinum wire wind immediately round the wick, the effect cannot be accomplished, and then it cannot be pronounced unequivocal in every case; my last appendage is calculated to supply the desideratum.

By clearing away the rubbish, I discovered a *vein* of the "polishing powder" traversing *gneiss*. It appeared in joints varying in length from one tenth to one inch: the vein was slightly inclined to the horizon; the silken fibres disposed longitudinally, and sometimes having their disposition altered by fragments of quartz interspersed with needle schorl. A more minute account will appear in my paper on the mineralogy of the Isle of Man, to be shortly submitted to the Wernerian Society of Edinburgh. I think it intermediate between *actinolite* and *asbestine tremolite*. I made more minute inquiry respecting the meteorolite of Pulros. It fell twenty-five years ago during a tremendous thunder storm, burst in its fall, and scattered its fragments over an area of three hundred yards in circumference: this space was ploughed up by the effects of the lightning in a zigzag manner, and the ridges were tinged of a *blueish* colour. The animals seem to have been killed by the *lightning* which preceded the descent of the meteoric stone, exhibiting but slight evidence of external injury.

I have the honour to be, &c.

Whitehaven, Nov. 21, 1817. J. Murray.

GEOLOGY.

The island of Great Britain presents a richer field for geological inquiry than any other country that has yet been examined, comprising in a comparatively shorte x[illegible]nt a succession of all theprincipal rocks, from those which have been regarded the most ancient to the very newest formations. There is scarcely any one species of rock of importance, except those of recent volcanic origin, that may not be found well characterized in some part of our island. Our mineral treasures, too, far exceed in annual value those of any country on the continent of Europe. With these inducements for research, and the great facilities that our insular situation affords for the study of geology in the bold and well

well defined sections which our coasts frequently present, yet the progress of the science has been hitherto greatly impeded by the variety of names given to the same species of rock, and from the want of characteristic and well arranged specimens. Many persons who have felt the truth of these remarks, and have read Mr. Bakewell's "Introduction to Geology," or attended his lectures, have repeatedly requested him to supply them with such rock specimens and descriptions as might enable them to pursue the study. He has therefore been induced to devote a considerable time to visit distant parts of our islands, purposely to select a series of instructive specimens in order to form geological collections, showing the principal rocks in their most characteristic form, and also their gradations and transitions into each other. These collections are accompanied with a descriptive catalogue by Mr. Bakewell, which, besides containing the names of each rock as given by the English, French, and German geologists, and marking their localities, will also notice peculiarities that serve to elucidate any striking fact in the science, or have a reference to prevailing theories.

Collections varying in size and value may be had by applying to Mr. Bakewell at his house, 13, Tavistock-street, Bedford-square.

ŒCONOMY OF FUEL.

The gentleman who sent us the article inserted in our last under this head, writes us, that he had then only speculated on the possibility of the balls which he recommended answering the intended purpose; but having since tried them, he finds that, with the proportions therein stated, they resist combustion.

BOILING SPRINGS OF JAVA.

The Penang Gazette of Feb. 10, 1816, contains the following article on the volcanic springs of boiling mud in Java:

"Having received an extraordinary account of a natural phænomenon in the plains of Grobogna, fifty paals north-east of Solo; a party set off from Solo, the 25th Sept. 1814, to examine it.—On approaching the dass or village of Kuhoo, they saw between two topes of trees a plain, an appearance like the surf breaking over rocks with a strong spray falling to leeward. Alighting, they went to the 'Bluddugs,' as the Javanese call them. They are situated in the village of Kuhoo, and by Europeans are called by that name. We found them to be on an elevated plain of mud about two miles in circumference, in the centre of which, immense bodies of soft mud were thrown up to the height of ten to fifteen feet in the form of large bubbles, which, bursting, emitted great volumes of dense white smoke. These large bubbles, of which there were two, continued throwing

ing up and bursting seven or eight times in a minute by the watch—at times they threw up two or three tons of mud. They got to leeward of the smoke, and found it to stink like the washings of a gun-barrel.—As the bubbles burst, they threw the mud out from the centre, with a pretty loud noise, occasioned by the falling of the mud on that which surrounded it, and of which the plain is composed. It was difficult and dangerous to approach the large bubbles, as the ground was all a quagmire, except where the surface of the mud had become hardened by the sun:—upon this we approached cautiously to within fifty yards of one of the largest bubbles or mud-pudding as it might properly be called, for it was of the consistency of custard pudding, and was about one hundred yards in diameter:—here and there, where the foot accidentally rested on a spot not sufficiently hardened to bear, it sunk—to the no small distress of the walker.

"They also got close to a small bubble, (the plain was full of them, of different sizes,) and observed it closely for some time. It appeared to heave and swell, and, when the internal air had raised it to some height, it burst, and the mud fell down in concentric circles: in which state it remained quiet until a sufficient quantity of air again formed internally to raise and burst another bubble, and this continued at intervals of from about half a minute to two minutes.

"From various other parts of the pudding round the large bubbles, there were occasionlly small quantities of sand shot up like rockets to the height of twenty or thirty feet, unaccompanied by smoke:—this was in parts where the mud was of too stiff a consistency to rise in bubbles. The mud at all the places we came near was cold.

"The water which drains from the mud is collected by the Javanese, and, being exposed in the hollows of split bamboos to the rays of the sun, deposits crystals of salt. The salt thus made is reserved exclusively for the use of the Emperor of Solo: in dry weather it yields thirty dudgins of 100 catties each, every month, but, in wet or cloudy weather, less.

"Next morning we rode two paals and a half to a place in a forest called Ramsam, to view a salt lake, a mud hillock, and various boiling pools.

"The lake was about half a mile in circumference, of a dirty-looking water, boiling up all over in gurgling eddies, but more particularly in the centre, which appeared like a strong spring. The water was quite cold, and tasted bitter, salt, and sour, and had an offensive smell.

"About thirty yards from the lake stood the mud hillock, which was about fifteen feet high from the level of the earth. The diameter of its base was about twenty-five yards, and its top

about

about eight feet—and in form an exact [illegible]. The top is open, and the interior keeps constantly boil[illegible] heaving up like the bluddugs. The hillock is entirely formed of mud which has flowed out of the top;—every rise of the mud was accompanied by a rumbling noise from the bottom of the hillock, which was distinctly heard for some seconds before the bubble burst;—the outside of the hillock was quite firm. We stood on the edge of the opening and sounded it, and found it to be [illegible] fathoms deep. The mud was more liquid than at the bluddugs, and no smoke was emitted either from the lake, hillock, or pools.

"Close to the foot of the hillock was a small pool of the same water as the lake, which appeared exactly like a pot of water boiling violently;—it was shallow, except in the centre, into which we thrust a stick twelve feet long, but found no bottom. The hole not being perpendicular, we could not sound it without a line.

"About 200 yards from the lake were two very large pools or springs, eight and twelve feet in diameter; they were like the small pool, but boiled more violently and stunk excessively. We could not sound them for the same reason which prevented our sounding the small pool.

"We heard the boiling thirty yards before we came to the pools, resembling the noise of a waterfall. These pools did not overflow—of course the bubbling was occasioned by the rising of air alone. The water of the bluddugs and the lake is used medicinally by the Javanese."

LITHOGRAPHY.

The art of lithography continues to make most rapid progress in France from the rival exertions of Count Lasteyrie and M. Engelmann: their spirited emulation has done for it what a monopoly would not have accomplished in a century. Under Count Lasteyrie's care, it rivals copper in almost every line of engraving, and possesses, besides, advantages peculiar to itself. A series of lithographic prints, by Count Lasteyrie, is now publishing in Paris, under the title of "A Collection of different Kinds of Lithographic Impressions, which may be advantageously applied to the Sciences, and the Mechanical and Liberal Arts." The second number, containing six plates, has just appeared; an account of them cannot fail to interest our readers. The first is the original design of a great master,—a pen-and-ink drawing, which is rendered with perfect fidelity and spirit. This plate offers, too, another species of interest, and that very important; the design has been traced on the stone upwards of sixteen years, and the proofs are as fine and spirited as if it had not been done so many days. This is a triumphant proof that lithographic designs upon stone may be kept any length of time, like a copper-plate.

plate. The second plate is a pencil-drawing of a plant; we have seen an engraving of the same plant in a botanical work of great luxury of execution, and we hesitate not to prefer the lithographic impression. The third plate presents various specimens of writing—Italic, Roman, &c. and fac-similes of old Greek manuscripts. In this department the lithographic art is unrivalled; it presents the originals with an accuracy in every way that it is impossible for any other branch of art ever to attain. The fourth plate is a topographic plan cut in stone, which produces a very striking and peculiar effect. The Count Lasteyrie's Battle of Austerlitz may be cited as a model of perfection in this way. The fifth plate is a pencil-design of a nosegay of roses: lithography seems excellently calculated to render with truth the various parts of flowers with a softness and precision resembling nature. The sixth plate is written music, or, as the lithographers denote it, *autographed music*. The method by which this plate is executed displays one of the most important advantages of lithography:—a person writes a letter, composes music, or makes a drawing on paper in the ordinary way, excepting that he uses a peculiar ink; this is transferred to the stone by simply passing it through the press, and the stone, without further preparation, is ready to print off thousands of proofs, all equally perfect. It is this quality of lithography that has secured its admission into all the French public offices: by its means 60,000 or 70,000 proclamations, in the autograph of the minister, may be taken off and dispatched before the plate even could be engraved. In the branch of landscape, the Count Lasteyrie has recently surpassed his former efforts so far that they will not bear any comparison with each other: it is difficult to fix the limits of genius, united with application, or we should be inclined to believe that he had very nearly attained the perfection at which it is possible for the art to arrive.

LECTURES.

Mrs. Lowry, whose lessons in Mineralogy are exemplified by a systematically arranged and extensive collection of minerals and models, will recommence her instructions directly after Christmas, at her house, 57, Great Titchfield-street.

Mr. Taunton's Winter Course of Lectures on Anatomy, Physiology, Pathology, and Surgery, will commence at the Theatre of Anatomy, Hatton Garden, on Saturday, January 24, at eight o'clock in the evening precisely, and be continued every Tuesday, Thursday, and Saturday, at the same hour.

Mr. Thomas Bell, F.L.S. will commence his Lectures on the Structure and Diseases of the Teeth, &c. at Guy's Hospital, on Friday the 9th of January, at half past five o'clock. Tickets may be

be obtained by applying to Mr. Stocker, at the Hospital, or to Mr. Bell, 17, Fenchurch-street.

Mr. Guthrie, Deputy Inspector of Military Hospitals, will commence his Spring Course of Lectures on Surgery, on Monday, the 19th January, at Five Minutes past Eight in the Evening, in the Waiting Room of the Royal Westminster Infirmary for Diseases of the Eye, Mary-le-bone Street, Piccadilly. To be continued on Mondays, Wednesdays, and Fridays.

Medical Officers of the Navy, the Army, and the Ordnance, will be admitted gratis, on obtaining a recommendation from the Heads of their respective Departments, which must be presented to Mr. Guthrie between the hours of Two and Half-past Four, at his house, No. 2, Berkeley Street, Berkeley Square.

LIST OF PATENTS FOR NEW INVENTIONS.

To William Harry, of Morriston, near Swansea, Glamorganshire, for his improvement in the building, constructing or erecting the roofs or upper parts of furnaces used for the smelting of copper and other ores, or any of their metals, or for any other purposes requiring strong fires.—3d Oct. 1817.—2 months.

To John Oldham, of South Cumberland-street, Dublin, for his improvements in the mode of propelling ships and vessels on seas, rivers, and canals, by the agency of steam.—10th Oct.—6 months.

To Robert Dickinson, esq. of Great Queen-street, Lincoln's-inn-fields, Middlesex, for his improvements in sea-beacons and their moorings.—1st November.—6 months.

To Frederick Dizi, of Crabtree-street, in the parish of Fulham, Middlesex, for his improvements on harps.—1st November.—6 months.

To Francis Marcellin Molle, of Bucklersbury, in the city of London, merchant, in consequence of a communication made to him by a certain foreigner residing abroad, of certain improvements in propelling boats and other vessels.—1st November.—6 months.

To Henry Meade Ogle, esq. of Turnham-green, Middlesex, for his improvements in and on tea and coffee biggins.—1st November.—6 months.

To George Clymer, late of Pennsylvania, but now of Cornhill in the city of London, mechanic, for certain improvements in writing presses.—1st November.—6 months.

To Thomas Curson Hansard, of Peterborough-court, Fleet-street, in the city of London, printer, for his improvements on and additions to printing presses, and also in the processes of printing.—1st November.—6 months.

To

To Daniel Towers Shears, of Fleet-market, in the city of London, copper-smith, for his invented machine for the cooling of liquids, which may be applied to the condensation of vapour, and may be of great utility in the condensing of spirits in the process of distillation, and in cooling worts, beer, and other liquids.—1st November.—2 months.

To Samuel Hall, of Basford, Nottingham, cotton-spinner, for his method of improving thread or yarn, as usually manufactured, of every description, whether fabricated from flax, cotton, wool, silk, or any other vegetable, animal, or other substance whatever.—3d November.—6 months.

To S. Hall, (as above) for a certain method of improving every kind of lace or net, or any description of manufactured goods whose fabric is composed of holes or interstices made from thread or yarn, as usually manufactured, of every description, whether fabricated from flax, cotton, wool, silk, or any other vegetable, animal, or other substance whatsoever. — 3d November. — 6 months.

To Joseph Claude Niepee, of Frith-street, Soho-square, for an invention communicated to him by his brother, Joseph Niepee, a foreigner resident abroad, with certain additions of his own, of certain improvements in the means of propelling boats and other vessels, and which improvements are also applicable to machinery of various descriptions.—25th November.—6 months.

To Francis Baisler, of Oxford-street, for certain improvements on machinery used for cutting paper, which he intends to denominate Baisler's Patent Plough.—28th Nov.—6 months.

To John Hague, of Pearl-street, Spital Fields, for certain improvements in the method of expelling molasses or syrup from sugars, and also in refining of sugars.—28th Nov.—6 months.

To John Turner, of Birmingham, for certain improvements in the plating copper or brass, or a mixture of copper and brass, with pure or standard gold, or gold mixed with a greater portion of alloy; and in the preparation of the same for rolling into sheets.—5th December.—6 months.

To William Buck, of Ponsburn Back, in the county of Hertford, and Robert Harvey, of Epping, in the county of Essex, for certain improvements in the means or mode of making pipes and tubes of porcelain clay or other ductile substances.—5th December.—6 months.

To Mr. Stratton, of Gutter-lane, Cheapside, for improvements in certain part or parts of gas apparatus.—5th December.—6 months.

To Joseph Wild, of Pylewell House, in the county of Southampton, for a machine for separating corn, grain, and seeds from the straw.—5th December.—2 months.

 To

To Stephen Price, of Stroud, in the county of Gloucester, engineer, for his improved substitute for teasles to be used in the dressing of woollen cloths or fabrics which require dressing.—5th December,—2 months.

To Moses Poole, of Lincoln's-Inn, for certain improvements in steam engines, communicated to him by a certain foreigner residing abroad.—15th December.—6 months.

To Jean Frederick, Marquis de Chabannes, of Drury-lane, Middlesex, for certain improvements upon the inventions for which letters patent were granted to him, the first bearing date the 16th day of January in the 55th year of his majesty's reign, and the second bearing date the 5th day of December in the 56th year of his reign; which improvements are applicable to the purposes of warming, cooling, and conducting air in houses and other buildings; and also of warming, cooling, evaporating, conducting, and taking the residuum from liquids, and to other useful purposes, partly of his invention, and partly communicated to him by a foreigner residing abroad.—19th Dec.—6 months.

To Jean Frederick, Marquis de Chabannes, of Drury-lane, for his new method of constructing pipes or tubes of copper, sheet lead, sheet iron, tin, or other metals or mixture of metals, capable of being reduced into sheets.—19th Dec.—2 months.

To John Lewis, Wm. Lewis, and William Davies, all of Brimscomb, in the county of Gloucester, for certain improvements on wire gig mills for the purpose of dressing woollen and other cloths that may require such process.—19th Dec.—6 months.

To Arthur Howe Holdworth, of Dartmouth, Devonshire, for an improvement or improvements on gazometers.—2 months.—19th Dec.

ASTRONOMICAL PHÆNOMENA, JANUARY 1818.

D. H. M.		D. H. M.	
1.10.21	☾ κ ♍	16. .	☾ in apogee
1.14.29	☾ λ ♍	17.15.26	☾ ♂
2. 4. 7	☾ α ♎	18. .	♀ 776 Mayer * 7′ S.
3. .	♃ ♀ in contact	18.18.46	☾ 125 ♉
3. 8.53	☾ δ ♏	20. 1.42	☉ enters ♒
4.13.25	☾ θ Ophiuchi	20.21.30	☾ υ ♊
5. .	☾ in perigee	23.18.1	☾ η ♌
7. .	♄ σ ♒ * 26′ N.	26.21.46	☾ γ ♍
8.16. 6	☾ ε ♑	27.11.52	☾ θ ♍
9. .	♄ 58 ♒ * 8′ N.	28.20.20	☾ λ ♍
11.14. 9	☾ 29 ♓	29.10.20	☾ α ♎
11. .	♀ 740 Mayer * 18′ S.	30.14.14 Im ⎫	δ ♏ 6 N. of ☾
14 .	♉ stationary	30.15.15 Em ⎭	Cent.
14. .	♂ stationary		

A BRIL-

A BRILLIANT METEOR.

Ipswich, Dec. 18, 1817.

SIR,—It may not be uninteresting to some of your readers to have an account of a brilliant meteor I observed on the morning of the 8th instant, at three minutes before one o'clock. I was looking at Mars, whose position is near to the star in the bull's northern horn. About midway between the two horns I suddenly perceived a fiery body resembling a red-hot ball of iron, four or five inches in diameter, which having passed three or four degrees in a direction between the principal stars of Capella and Canis minor, burst into a spherical body of white light nearly as large as the full moon, of so great lustre as scarcely to be borne by the eyes, throwing out a tail about three degrees in length of a beautiful rose colour tinged round the edges with blue. It thus proceeded in its course without apparent diminution towards the principal star in the head of Hydra, (very near to the ecliptic,) a little beyond which it suddenly disappeared (I believe) with an explosion; as I distinctly heard a rumbling noise like that of cannon discharged at a distance, about ten or twelve seconds afterwards. Its duration as nearly as I can estimate was about five seconds, during which it traversed a space of nearly sixty degrees. It is scarcely possible to give an adequate description of the vivid splendour which characterized this extraordinary phænomenon. It cast a light around equal to the noon day's sun: I could compare it to nothing so well as the beautiful dazzling light exhibited by the combustion of phosphorus in oxygen gas; its effect upon the organs of light being analogous. The barometer was falling at the time, and in the course of the night fell altogether an inch and one tenth; the thermometer 42°. Within a quarter of an hour afterwards the atmosphere became entirely obscured by clouds; violent tempests of wind and rain succeeding, although the stars were previously visible and the zenith free from vapours. The short period elapsing during the meteor's course renders it difficult to be more particular in description, as it may scarcely be possible altogether to resist the influence of imagination upon a phænomenon so unexpected, so striking, and so beautiful. Astonishment and pleasure not unmixed with awe oppress the mind with a variety of reflections, not the least of which is our total inability accurately to account for these blazing wonders of the aërial regions.

As this part of Meteorology appears to be in its infancy, I should think it would be very desirable if a plan could be pointed out for more particularly observing and noticing these bodies; so that a series of tables might hereafter be made of them. I should suppose the distance of this meteor must have been about two miles, and height rather more than a mile and a half.

I am, Sir, &c. J. A.

Meteorological Journal kept at Walthamstow, Essex, from November 15 to December 15, 1817.

[Usually between the Hours of Seven and Nine A. M. and the Thermometer (a second time) between *Noon* and One P.M.]

Date.	Therm.	Barom.	Wind.
November.			
15	55 55	29·32	SE—NW.—Rainy; showers, sun and wind; bright star-light; a shower at 9½ P.M. Moon first quarter.
16	43 51	29·18	SW.—Hazy; sunshine; some rain after dark; dark, damp, and windy.
17	52 59	30·10	SW.—Hazy; damp hazy day; light, but neither moon nor stars visible.
18	52 57	30·21	SW.—Beautiful red sunrise; cloudy day; stars and *cumuli*.
19	39 50	30·34	NW.—Clear morn; very fine day; clear moon and star-light.
20	35 43	30·43	NW.—Foggy and white dew; sun through fog; light, but neither moon nor stars visible.
21	46 50	29·88	SW.—Gray morning; *cirrostratus* and clear; fine day; light, but no moon nor stars visible.
22	39	30·11	NW.—Fine sunrise; very fine day; cloudy, but *light*.
23	41 46	30·11	W.—Gray morning; gray day; cloudy. Full moon.
24	49 50	29·93	W.—Gray morning; gray day; slight rain; *cumuli*; stars and moon.
25	33 40	29·76	W—NW.—Clear moon-light morning; fine sunny day; some rain after 4 P.M.; cloudy, but light.
26	41 52	30·00	W—NW—SW.—*Cirrostratus;* ground very wet; fine day; light, but no moon nor stars visible.
27	47 52	30·10	SW.— Gray morning; and gray day; very dark at 3 P.M.; *corona* round the moon, and stars.
28	46 50	30·10	NW.—Gray; fine day; cloudy.
29	48 53	30·01	S.—Hazy; foggy; gleams of sun; fine day; dark night.
30	51 54	29·98	S.— Wind and showers; hazy day; dark night.

December

December

*1	53 54	29·88	SW.—Rain; very rainy day; rain till after 9 P.M.; very dark night. Moon last quarter.
2	45 48	29·66	SW—N.—Rainy till about noon; light orange sunset; star-light.
3	32 40	29·54	NW.—Clear at 7 A.M.; at 8 slight snow and rain; sun and showers; fine star-light.
*4	30 37	29·88	N—SW.—Clear, and white frost; fine day; foggy at 6 P.M.; 7 P.M. star-light; 9 dark; at midnight star-light.
5	43 45	29·86	SW.—Cloudy; cold gray day; dark night.
6	39 43	29·43	SW—NW.—Very damp and foggy; fine day; sun and *cirrus*; bright star-light.
7	33 41	29·43	SW—W.—Foggy, and white frost; sun and some showers; star-light.
8	37 45	28·73	SW.—Clear and *cirrostratus*; foggy; showery; very dark night. New moon.
9	46 48	29·90	NW.—*Cirrostratus*, and wind; fine day; *brown* orange sunset; star-light.
10	30 36	29·21	NW.—Clear; and white frost; some snow and rain before 10 A.M.; fine day; star-light.
11	24	29·43	SW.—Clear; white frost; fine day; clear star-light.
12	24 29	29·64	SE.—Foggy; fog very thick at 9 A.M.; sun through fog; dark night; some rain and snow.
13	39 41	29·55	E—SE.—Cloudy; cold cloudy day; and small rain; rain.
14	41 45	29·53	SE.—Damp, and cloudy; small rain almost all day; rainy; storm, hail and wind at 9 P.M.
15	30 43	29·54	W—SW.—Fine clear star-light morn; fine day; showers afternoon; stars; moon and stars. Moon first quarter.

*** 1st and 4th of December grass in the garden *mowed* — not common *after* October.

METEORO-

METEOROLOGICAL JOURNAL KEPT AT BOSTON, LINCOLNSHIRE.

[The time of observation, unless otherwise stated, is at 1 P.M.]

1817.	Age of the Moon	Thermometer.	Barometer	State of the Weather and Modification of the Clouds.
	DAYS			
Nov. 14	5	[illegible]	29·60	Rain
15	6	55	29·49	Ditto
16	7	53·	30·	Very fine
17	8	57·5	30·30	Fair—the air very damp
18	9	56·	30 26	Fine
19	10	49·	30·53	Cloudy
20	11	44·	30·48	Very fine
21	12	51·	29·94	Ditto
22	13	46·	30·23	Ditto
23	full	48·	30·15	Ditto
24	15	48·	29·85	Cloudy
25	16	39·	29·94	Fine
26	17	50·	30·12	Cloudy—rain A.M.
27	18	51·	30·15	Ditto
28	19			Very fine
29	20	55·	29·98	Ditto
30	21	53·	30·05	Cloudy
Dec. 1	22	35·	29.83	Ditto—severe frost in the morning
2	23	46·	29·63	Fine
3	24	41·	29·66	Rain—h[illegible]storm P.M.—rime frost
4	25	38·5	30·83	Very fine
5	26	46·	29·71	Cloudy
6	27	41·	29·26	Fine
7	28	44·	29·46	Cloudy—heavy rain A.M.
8	new	40·	28·71	Fine—rain P.M.
9	1	39·	29·26	Fair
10	2	35·	29·39	Fine—rime frost
11	3	33·5	29·15	Very fine—ditto severe
12	4	32·	29·80	Ditto—ditto ditto
13	5	38·	29·78	Cloudy—rain P.M.
14	6	43·	29·63	Rain
15	7	44·	29·72	Very fine

METEORO-

METEOROLOGICAL TABLE,

By Mr. Cary, of the Strand,

For December 1817.

Days of Month.	Thermometer: 8 o'Clock, Morning	Thermometer: Noon.	Thermometer: 11 o'Clock, Night	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
Nov. 27	46	52	46	30.08	9	Cloudy
28	47	50	47	·02	16	Fair
29	48	55	50	29·92	0	Small Rain
30	52	55	54	·90	0	S. Rain
Dec. 1	55	55	50	·72	0	Rain
2	48	52	40	·42	6	Cloudy
3	35	39	38	·53	10	Showery
4	31	37	39	·72	10	Fair
5	43	46	42	·58	9	Cloudy
6	43	44	37	·35	14	Fair
7	38	43	39	·31	13	Fair
8	40	45	38	28·51	0	Stormy
9	38	42	35	29·17	6	Cloudy
10	32	37	30	·46	0	Snow
11	27	35	28	·50	8	Fair
12	25	35	40	·60	9	Fair
13	40	42	40	·58	0	Rain
14	40	47	[illegible]	·42	0	Rain
15	38	47	42	·60	10	Fair
16	42	50	47	·61	0	Stormy
17	42	45	40	·60	14	Fair
18	43	46	40	28·84	9	Fair
19	40	46	42	·78	0	Showery
20	42	42	36	29·50	7	Cloudy
21	32	37	34	·40	9	Cloudy
22	31	35	35	·45	12	Fair
23	28	35	30	·46	12	Cloudy
24	28	34	31	·70	14	Fair
25	29	36	29	·90	17	Fair
26	27	33	[illegible]	30·09	[illegible]	Fair

N B. The [illegible] is taken at one o'clock.

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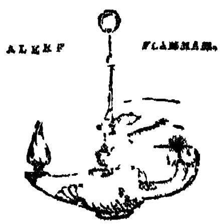
END OF THE FIFTIETH VOLUME.



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